MOTHER TONGUE

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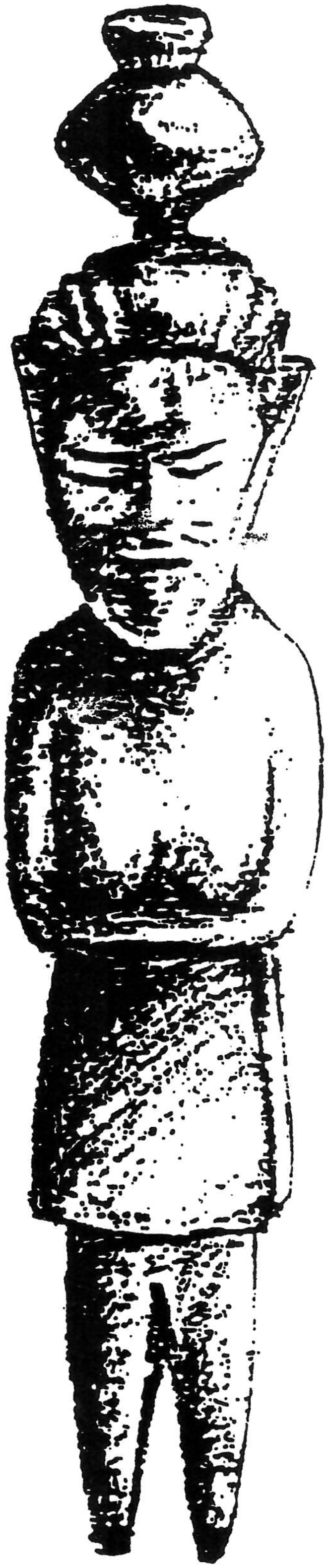

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Mother Tongue: The Journal

We regret very much the death (in February) of Frank Livingstone, Emeritus Professor of Anthropology at the University of Michigan. Not only was Frank a genuinely important contributor to physical anthropology and African prehistory, he was a friend of many years standing. He was also a 'Landsman' or countryman of Dan McCall and me, hailing from eastern Massachusetts and maintaining the strange dialect and political outlook of that region. Dan and I came from the western or 'hard r' part of New England, so Frank's 'soft ah' pronunciation distinguished him from us. Well, not politically. As a member of the founding generation of Long Rangers, Frank stayed with us all the way.

Frank was given an oral Festschrift at the AAPA meetings in Buffalo a few years ago and we presume that there will be long, detailed obituaries in several scientific journals. We doubt that we can add much to what has already been placed on the public record. We will restrict ourselves to general comments and Africana not so likely to be included in those other summaries which will be dominated by biological anthropological considerations.. We begin with Dan McCall's brief but salient comment.

> Frank Livingstone first came to my attention with his article on the distribution of the frequencies of Haemoglobin S among the various peoples in West Africa. He found that the high rates were among peoples who were yam cultivators in the forest. The farmers killed trees to create yam fields. This resulted in pools of standing water after rains, permitting mosquitoes to breed and flourish, thus increasing the vector of malaria. The Haemoglobin S gene gave some protection against malaria. Where the S gene was highest, one might deduce, was likely to be the area where the gene first appeared, because it would take generations for the gene to multiply in a population. Thus one could say with some assurance that yam farming in West Africa began in Nigeria and spread from there. The lowest rate of the S gene was in the geographical location of Liberia.
>
> This fascinating contribution of population genetics to the prehistory of West Africa led me to delve into the literature of this discipline and I thank Frank for stimulating me to do so. (Signed) Daniel F. McCall, Professor Emeritus (Anthropology), African Studies Center, Boston University.

My own most vivid memory concerns one of Frank's contributions – the case of the genetic affiliations of the tall Tutsi of Rwanda-Burundi, lately the objects of Hutu genocidal efforts. Frank was clearly identified with the anti-Hamitic theory and analyses directed at turning back the waves of conquering cattle-herding Caucasoids so beloved of 19th century and early 20th century Euro-American theorists of prehistory. While Joseph Greenberg was influential in the same struggle, his efforts centered on linguistic and cultural analyses. Frank's primary target tended to be the concept of Race and its misuse in the reconstruction of African, indeed global, prehistory. As a 'Caucasoid' cattle people, indeed a ruling caste, deep in East Africa the Tutsi should have been like raw meat to a crusading anti-Hamiticist. He would be expected to show that the Tutsi were entirely a product of local Negritude, especially since they unquestionably spoke a Bantu language. But Frank was a scientist first, not an idealogue. He wrote a careful article on blood groups, comparing many East and West African groups, and stated plainly that the

Tutsi's closest affiliations were with the "Saharan peoples". As was shown later on, these were such as the Tuareg, Fulani, Teda, and Nubians, representing three different linguistic phyla but all similar enough genetically to be related to the Ethiopids of the Horn of Africa. Contrary to G.P. Murdock's conclusion that the Tutsi (and Hima) were descended from the Lwoo invasion of Uganda from the Sudan, Frank helped me to conclude that the Tutsi were the locals, Cushites whose language had been overwhelmed by the in-migrating Bantu. This point was later nailed down by Chris Ehret who proposed South Cushitic loan words in Lacustrine Bantu languages.

Frank's work also helped immensely with another 'thought revolution' for African prehistory. The overwhelming assumption until fairly recently was that the primordial population of Africa was 'Negro' everywhere south of the Sahara, and also in the settled areas of the Sahara itself. A later modification of that presumption occurred in eastern Africa where a countervailing assumption of 'Bushman' priority competed with Negritude. But in any case the assumption that the Bantu were the natives of East Africa, at least south of Ethiopia, and the Cushites or other 'Hamites' were intrusive, was the dominant one. G.P.Murdock was a substantial contributor to the changing belief that the 'Hamites' had been resident in East Africa, and therefore not restricted to the Horn, before the main in-migrations of Negroes from the Sudan (Kuliak and Nilotes) and later from West Africa (the Bantu). Throughout these changing mind sets, however, the assumption of a Bushman priority in time in eastern Africa has gone from co-dominant to dominant.

One probable consequence of the belief in the primordial Negro all over Africa has been its adoption by much of the field of population genetics. Their belief seems set in concrete by now -- that any genetic connections between sub-Saharan Africans and Middle Easterners is due to in-migration, if not actual dominance and conquest, by Mediterranean-looking folks. We , or at least most professional Africanists and prehistorians, now know this belief to be closely related to the old conquering Hamite bit. We don't believe it anymore. Frank Livingstone was unfortunately not able to dissuade his colleagues on this point.

But **Herbert S. Lewis** has something to add to our memory of Frank Livingstone, primarily from the viewpoint of ethnology.. Lewis is Emeritus Professor of Anthropology at the University of Wisconsin, former director of its African studies program, and long term student of African prehistory and history. He points out that Frank was not only instrumental in curbing the concept of race in African studies, where he is regarded as one of the most persuasive voices in that effort, but Frank put a considerable amount of effort into supporting the 'four fields' approach to historically-oriented anthropology. Frank's steady support of ASLIP and *MOTHER TONGUE* surely are aspects of that strong support.

It is an open secret that geneticists generally, as opposed to biological anthropologists, are reluctant to give up the concept of race. A recent (2004) conference devoted to that disagreement was convened in Toronto, Canada. As reported in *NATURE* (431: 1026), it was evidence of that. Neither side succeeded in converting the other, apparently.

Ah, Frank, my lad, we do surely miss you!

We regret to announce the recent **death of Sergei Starostin**. It was utterly unexpected and most of our members have not heard of this sad event as yet. He died on the last day of September in Moscow after teaching his class at Moscow State University. It was almost exactly nineteen years after the very beginning of ASLIP in Moscow.

We are told that it was 'heart failure' which seems to be a misnomer for a bursting aneurysm on his heart, a heart that had apparently been damaged by a previous undetected heart attack. I guess we can say, as the Americans do, that he died with his boots on, doing the thing he loved to do and was good at.

Only one of those who heard the bad news about Sergei was able to control his grief long enough to write a short tribute to him for this issue of MOTHER TONGUE. Others were unable to write a vital summary at this time or they were too busy during the little time we could allow them. Before we turn to the one who did manage to write briefly – despite his personal grief – the editor wants to say a bit on behalf of ASLIP. The understanding is that the next issue of MOTHER TONGUE will have a fuller section devoted to tributes to Sergei.

On behalf of ASLIP I must say that we have lost a most important member of our army. Short of Greenberg himself, Sergei's taxonomic contributions were very important to our progress. Although he shared the honor of 'inventing' Dene-Caucasic or Sino-Caucasic with Sergei Nicholaev and the leadership of the passionate young Russian linguists with Alexander Militariev and Vitalij Shevoroshkin, he was unmatched in his contributions to linguistic dating or what I came to call 'Sergechronology'. He could be very charming and as Vitalij Shevoroshkin once told me: "...he even is a nice guy!".

Our condolences to the Santa Fé Institute and Dr. Murray Gell-Mann. I am told that Sergei was the driving force behind their linguistic projects for the past several years and will be very difficult to replace – if that is at all possible! On behalf of ASLIP and our President, Profesor Michael Witzel, we extend our best wishes to Murray and his Institute and hope that we will be able to cooperate with them in the future on various projects, particularly since some of our members are now wearing two hats.We will be pleased to help Murray in his search for the unique candidate, the one who can replace the irreplaceable Sergei.

We do understand that Murray, although a friend and admirer of Joe Greenberg, was enamoured of reconstruction and Indo-European verities & virtues. Therefore we do not recommend a Greenbergian type scholar as Sergei's replacement. We are unable to make a long list because Sergei's type of talent is not common. But we recommend five scholars who are not vetoed because of personality impediments, age, comfortable situation, or known disinterest in the job. Our nominees are: **John D. Bengtson; Sergei Nicholaev**, if he has learned English; **Alan Bomhard** (Nostraticist); **Paul Black** (Afrasian, Australian); **Vaclav Blažek** (Eurasian, Near East antiquities, semi-global)

Good luck in your hunt and in your future research projects.

The man who was able to master his strong grief and write a short impromptu tribute to Sergei Starostin was John D. Bengtson, Vice-President of ASLIP

His letter follows overleaf.

Sergei Anatolyevich Starostin, 1953-2005

Sergei Anatolyevich Starostin, a major leader of the Moscow school of historical linguistics, Council Fellow of ASLIP, and Co-director of the Santa Fe Institute Evolution of Human Language Project, died suddenly on September 30, 2005, at the Russian State University for the Humanities in Moscow.

As I write this, less than a week after the dreadful news reached me, I am still in shock, stunned by the sudden departure of my colleague and friend. By the law of averages, Sergei should have outlived me by five years. Instead he joins Edward Sapir and Vladislav Illich-Svitych among the great scholars taken from us long before their time. This is not the time and place to enumerate Sergei's extensive contributions. That will wait for a later issue, when the sting of his passing has begun to heal, and we have had some time to reflect.

Sergei's many friends remember him as an uncommonly good man. Though his research often led him to promote controversial theories, he was not at all a combative personality. Yes, he argued mightily for the scientific methods of historical linguistics, as he knew them, and the results they led to. At the Santa Fe Institute he took part in many vigorous discussions, especially with us Americans whose methods, at least initially, derived more from Joseph Greenberg than from Illich-Svitych. Yet on a personal level he always remained friendly and cordial, and at the end of the day we might be enjoying dinner with Sergei, his wife Natasha, and son George. I believe these discussions led to an increase of mutual respect between the Nostraticists and the Greenbergians and, to some extent, a rapprochement and narrowing of the ideological gap that once existed.[1]

I am sure the members of ASLIP join me in wishing peace to Sergei's soul and comfort to his family and friends.

John D. Bengtson
Savage, Minnesota, U.S.A.

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[1] One must note that this process was a continuation of the dialog initiated in the 1980s by Aharon Dolgopolsky, Harold Fleming, and Vitaly Shevoroshkin.
[Or to be more precise: at the Moscow meetings of the Ethiopian Studies Association in August, 1986, Alexander Militariev, followed by Olga Stolbova, Anna Belova, and Viktor Porkhomovsky, began telling Harold Fleming about their work and their teacher, Dolgopolsky, and the 'sainted' Illich-Svitych. The young Russian group quickly grew larger and soon included Sergei Starostin whose work in glottochronology was praised by Militariev. They passed on to Fleming an article on Hurrian written by Igor Diakonoff and Starostin; this aroused Afrasian comparisons in Fleming. That is the exact beginning of what grew up to be the Association for the Study of Language In Prehistory. –ED]

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Joseph H. Greenberg Memorial Service
Stanford Memorial Church
October 23, 2001

Every year Joe would come to my class and lecture on some topic and in introducing him to my class I used to like to tell the story of what happened after Joe became famous for the African classification in the 1950's. What happened was the historical linguists realized that he had not really done this right. He had not reconstructed all the stuff, he didn't have regular sound correspondences, and he hadn't jumped through all of the burning hoops. Yet as people looked at the information more and more it became obvious that the answer was correct even if he had not done it correctly. And this posed a serious problem. How can a person solve an extremely complex problem and do it completely wrong? Nobody knew the answer for a while and then finally one day some linguist realized that, "look, Greenberg's a genius. He can just jump to conclusions like this, but we're normal linguists. We have to follow the real comparative method. We have to reconstruct; we have to do sound correspondences; we're no Greenberg." I liked telling this story because nothing drove Joe up a wall more than the idea that his African classification was successful because he was some kind of genius. His idea was the exact opposite. The African classification was not some mammoth genius thing, rather it was very simple. You simply compared words. It was so simple that anybody could do it and the students in my class had already solved African tables of words exactly as Joe had, so they realized that even without linguistic background they had been totally successful in classifying the African languages. They realized that Joe's idea that anybody could do this, if you simply used common sense, was in fact correct, and the idea that only geniuses could do this was in fact not correct.

Now at this time several other interesting things happened. First of all, I think Joe's genius was characterized by the ability to ask very simple questions—questions so simple that anybody could understand them, even non-linguists—and by asking these very simple questions he was able to arrive at extremely profound conclusions, extremely profound findings including the African classification, which was called by Murray Gell-Mann one of the great scientific discoveries of the entire twentieth century. There's also something else I think very startling about the African classification, and what really set Joe apart from most other scholars, and that is that we normally think of science as building on the work of others and at the time that the African classification was being done in the 1950's, early '50's, there were in fact two other scientific areas in focus, the first trying to understand the structure of DNA. Here we had fierce competition between different people, including Linus Pauling at Caltech and Watson and Crick, who obviously won this race. We also had in physics Murray Gell-Mann trying to work out the underlying structure of the atom and there were other people doing this too. In the African classification, in contrast, there was nobody else classifying African languages but Joe. There was probably not even anybody else who thought it was possible to classify African languages. He was doing something that nobody else had done or was doing.

Another interesting aspect of the African classification was that Joe was already internationally famous for the African classification by the time he was forty years old. Most academics who become internationally famous remain in that field and bask in the sunshine for the rest of their lives. Joe did not do this. Joe left the African field at this time and went off and did totally different things in totally different areas. He wrote one or two articles about African linguistics later on, but by and large he was doing other things.

One of the things already mentioned here was that he wrote this paper on the order of meaningful elements, that is, if you take the subject, verb, and object you can arrange these in six possible ways. Joe asked which of these six possible ways are actually used in human language. And if you look at the particular order of subject, verb, and object, and the adjective and noun, or demonstrative and noun, are there correlations between these different structures? Once again he was asking simple questions—questions again so simple that anybody can understand them—and yet he arrived at this field called typology. He basically founded modern typology with this article, and I think this article is the most cited linguistic article of all time. I can think of no other article in the entire linguistic literature that has been cited as often as this article. So once again Joe's genius was to ask incredibly simple questions. This article he wrote could have been written two hundred years earlier, three hundred years earlier. There was nothing preventing somebody from getting 30 languages three hundred years ago and asking these simple basic questions and yet nobody did this until Joe did it in 1962. His genius was really characterized by asking simple questions and arriving at very profound conclusions in very different areas of linguistics. The African classification, for example, was based on similarities that were historically connected. The evolutionary process explained them. However, in typology typological similarities were not evolutionarily explained. They happened independently in different places so he was asking very different questions, explained by different means, and yet in both cases he arrived at profound conclusions. He also arrived at profound conclusions concerning the classification of New Guinea languages, also considered by most people to be unapproachable and certainly nobody was working on them any more than on African languages. Later on he classified all of the New World languages into only three families, one of his most controversial findings in spite of the fact that the American Indian classification is really rather straightforward and simple compared with the African classification. In fact Joe worked out the American Indian classification in under one year back in 1956 and actually gave a talk in 1956 at an Anthropological Congress and laid out what the actual classification is, whereas the African classification took him from 1948 until 1963 before he worked out all of these details.

One of the personal aspects of Joe's life which I think Will kind of mentioned in his talk, and which I always admired very much, was that Joe treated everybody equally. Whether he was talking with an illiterate peasant in northern Nigeria or a Nobel laureate in this country he treated all of these people, and everybody else, with the same respect and same dignity all the time. Joe was one of my best friends, as many of you know, and I know that he will always be missed by those who were fortunate enough to have known him.

The Wee Fossil Folk of Sunda-land: The Mystery Unfolds

The extraordinary discovery of very small hominid skeletons by field workers on Flores (Lesser Sunda islands circa 120° East in eastern Indonesia, roughly the size of Estonia or Denmark or Bhutan or the Dominican Republic) has led to much puzzling about their classification but even more the meaning or interpretation of these wee fossils for human prehistory in general. They have been formally named *Homo floresiensis* and informally *the Hobbits*. Their remains are now officially housed in Jakarta. In the literature they are being officially called – 'hominins' not hominids. Some have called their discovery the most important fossil find of the last half century.

The formal Linnean taxonomic system displays 'hominins', as follows:

Order Primates Linnaeus 1758
Suborder Anthropoides Mivart 1864
Superfamily Hominoidea Gray, 1825
Family Hominidae Gray, 1825
    Tribe Hominini Gray, 1825
    Genus Homo Linnaeus, 1758
        Homo floresiensis sp. nov.

We asked a dozen distinguished paleo-anthropologists for their comments on the problem. For a variety of reasons only a few responded; but we are very grateful to them. The commenters are:

David Pilbeam of Harvard University
Milford Wolpoff of the University of Michigan
Tim White of the University of California

And we will get to their remarks in a moment.

First, some more background. The first reports were in NATURE October, 2004. From the first, the problem was three-fold. Although the skeletons formed a population – most of one body and pieces of seven others and not all contemporaries– and thus reduced the probability of this being simply a highly aberrant individual, the lot of them were too late in human history, too tiny, and too sm all-headed to fit easily into *Homo erectus* and his lineage or *Homo sapiens* and his lineage. Secondly, the fossils ranged over 20 millennia – 13 kya to 34 kya – and included a range of human ages from teenagers to those over 70. Associated remains (bones and tools) reached back to 92 kya. Evidence of *Homo erectus* had once been found on Flores, datable to 800 kya. The prime focus of discussion has always been one adult female and her head, dated to 18,000 BP.

Early on, controversy sprang up. Initially, it concerned who really owned the fossils, the excavators from Australia or the Indonesians on whose land the fossils lived. Then a milder form of controversy, perhaps disagreement is a better term, about the relationship between the Hobbit brain size and those of modern *microcephalics.* Mind you, we are not talking about dwarves or Pigmies here. Their normal gross size is larger and their brains certainly larger. We are talking about Hobbit brains the size of or smaller than the Australopithicinae and our close primate relatives —circa 400-500 cc. Or half a liter. Which is totally outside of the range of modern man, except for the rare and basically diseased microcephalics, and quite low for Homo erectus (700cc-1100cc).

Since the average newborn human baby has a brain 30% of the average adult brain or 0.30 x 1500cc = 450cc, while an average 1-year old human baby has 60% of said adult brain or 0.60 x 1500 = 900cc, we can see that our Hobbits never achieve brains as large as human **babies**. (These numbers are from standard text books in Psychology and Neuroscience.) Except for the infant Anatole France whose birth brain (by these calculations) would have been smaller (0.30x1000cc) at 300cc than the Hobbits! Was he really so smart, this Jacques Thibeault, just because he won the Nobel Prize in Literature in 1921?? Should French literati high culture get some of the credit?

. Before we discuss other points of disagreement or uncertainty, let us hear from our experts:

David Pilbeam (Peabody Museum, Harvard University) first:

> The report last year in NATURE of a new species of fossil hominid made headlines for three reasons (Nature, 431: 1055-1061, 1087-1091). First, new hominid species always make the headlines. Second, this new species, *Homo floresiensis,* was strikingly young, living as recently as 18,000 (or 18 kyr) ago on the Indonesian island of Flores. Third, nicknamed "the hobbit", *H. floresiensis* was tiny and quite unlike all other populations of the genus *Homo* which for the last 2 million years, have been of living human body size..
>
> Several specimens, ranging in age from around 18 to over 70 years old, show that we are witnessing a population, rather than one or two aberrant individuals. Brain size is in the australopith range, around 400 cm^3 (= cc or cubic centimeters – ED.). This same individual stood 3'6" tall (= about 107 cm – ED) and may have weighed around 80 lbs (= 36.36 kilos –ED). Teeth were in the human size range, forelimbs were longer than in humans, and several other features of the postcranial skeleton, for example the way iliac blades flare to the side rather than curving forward, also differ from patterns we see in *H. sapiens*.
>
> The endocast (approximating the brain surface) has been more recently described (SCIENCE, 308: 242-245), and is most like (least unlike) *H. erectus*, although a number of surface features on the cerebral cortex "are consistent with higher cognitive processing" (language?). Archeological associations, or at least tools and bones found in the same place and time, suggest hunting of large game, and possible use of fire and cooking.
>
> What is this new species, or rather who were its ancestors? A separate australopith ancestry is very unlikely, so possibilities are that it is a dwarfed erectus, a dwarfed sapiens, or a dwarfed version of a post-erectus/pre-sapiens species. The basically erectus-like but derived endocast suggests to me the latter as the current best option. We know what "dwarfed " humans look like: diminutive humans with brains and heads barely smaller than normal-sized humans. It is not a human microcephalic – again, endocast morphology does not fit that hypothesis. Yet it is likely to be derived from a species that still responded in non-(modern) human ways to island isolation. It is typical of many animals in such situations to become dwarfs or giants.

> At the moment (= late May, 2005 - ED) the new species raises more questions than can be answered. All that one can say is that this was totally unexpected; how many more surprises are waiting for us?

With appreciation for David Pilbeam's contribution we turn, appropriately, to Tim White (University of California, Berkeley) because he suggests that there may be more, possibly including surprises. Tim as you know worked with Donald Johansen and his team on the discovery, excavation, and analysis of *Lucy*, arguably the most famous Australopithicine ever unearthed. Lucy was in fact about the same size as one of our Hobbits, maybe a kind of 'deja vue' to stimulate Professor White's memory. His comment was limited to this:

> ...Your June 15th deadline is long gone, and since there are new remains of this thing out there that will garner even more coverage/debate after being published (hopefully soon), I'm going to politely decline to comment in your journal –and get stuck in with a bunch of other things we've found in Ethiopia over the last decade. ...

(His last clause/sentence is delightfully obscure. Alas, the joys of e-mail! – ED))

Milford Wolpoff (University of Michigan, colleague of Frank Livingstone) has been most well-known in recent decades as a vigorous participant in the international debate between the 'Out of Africa' hypothesis and the 'Multi-Regional Co-Evolution' hypothesis' (to give it one of its several names). Besides attaching two articles from the media which greatly heightened the sense of controversy over the Hobbit Sundalanders, he enclosed his personal comments, offered here below:

> Thanks for writing, Hal. I only wish I had a better idea of what the Flores dwarf is about. I remain of mixed mind about whether it is a pathology (microcephaly in an otherwise small population) or some normal evolutionary descendant from an earlier population. I expect this would have to be some sort of australopithecine or habiline (cf *Homo habilis* – ED) who reached the region. The brain is too small to be a drafting of any *Homo sapiens* because such a process would require selection against brain size and all that comes with it, language, tool making, kinship, etc., and that is just too hard to imagine! I wonder if its not too early to write (at least for me)....

(I wonder what he meant by 'drafting' –ED) We can all agree, probably, that it is too early to decide what to write about the wee folk. However, it is ever thus in the fossil realm. Data are first acquired and then described and then thought about again and then discussed or debated and then classified again. It is as if we all were a giant brain thinking about a problem but all together. The more dissimilar the new data are in relation to older data and/or conclusions then the greater the difficulty in reaching collective decisions. It is that way in historical linguistics and many other branches of science too.

We do not need to get angry about the data or our colleagues' baffling disagreements with us, but that too happens a lot in science, although mostly it is concealed. One consequence of the Flores finds is that finally a nasty spat erupted between two Australian scientists about the matter, one a former mentor of the second, the former student. The issue between them was the taxonomic conclusion; one said it was *Homo sapiens* and the other said *Homo floresiensis*. Presumably the battle rages on.,

Of Wolpoff's two articles more interesting to me was the TIME magazine discussion of the modern wee people who live on the same island and claim the tiny people of the Liang Bua cave (excavated by the Australians) as their ancestors. (cf TIME, May 30, 2005). Many of the modern people, living in a village called Rampasasa (on Flores), were described as 'short' with the chief informant/story teller being a mere 140 cm. tall. Many remembered grandparents, supported by photos, who were much shorter, down to 110 cm for one. (= 4'7" down to 3'7" –ED). That lower number falls basically below the normal range of Pygmies and Southeast Asian Negritos too (I presume). But 140 cm would lie comfortably within the Pygmy range.

[In gross terms, after looking at many photos, we can say that normal Pygmy heads reach up to the **nipples** or **armpits** of ordinary Euro-American males, while Hobbits reach their **navels**. There are no stable data, as yet, on Leprechauns. –ED]

The Rampasasa informants or story tellers said four other arresting things. First, that their ancestors had been very short, shorter than Rampasasans. Second, that the ancient ones had lived in the same cave, Liang Bua, where the Hobbits were excavated. Third, that they hunted 'big game' but hardly knew how to cook it – without fire. Fourth, that the present Rampasasans descend from a marriage between those ancestral wee people and regular normal-sized human beings. Merveilleux! Can we get some DNA from the Rampasasans and off one of the fossils? (The excavators are trying to do that.). Better yet perhaps, should we wait for more research to tell us more?

Incidentally, the 'big game' mentioned above that the Hobbits hunted included a very large animal **dwarfed** even more than the Hobbits, namely the Stegodons or Pygmy elephants. I have seen films of Congo Pygmies hunting elephants (normal large ones) and it is easy to see how the feat could be accomplished; something like Lilliputians taking down an amused Gulliver. But the Hobbits' other game included those made much **larger** by island isolation, namely the lizards, mostly varanids and especially the venomous and aggressive Komodo Dragons which can reach 3 meters (10') in length. They are the largest lizards in the world and three times the length/height of our Hobbits who must have been very brave or foolhardy or very hungry to tackle such a nasty beast! It must have been a little bit like living with dinosaurs. (The Dragons are restricted nowadays to one island off the northwest coast of Flores.)

The archeology of Liang Bua is also highly unusual. Here is the rare case where the cultural debris is not necessarily associated with the hominin fossils. The problem derives from the taxonomy of the Hobbits, or the theory of Hobbit prehistory challenges any association with sophisticated human tool kits. How could the descendants of Homo erectus have the intelligence and technological background to make tools of 'Upper Paleolithic' type? What are the tools? Burin cores, macro-blades, bipolar cores, micro-blades, 'perforators' (awls, no? – ED) and indications of direct percussion using hammerstones. Those things are only made by fairly recent human beings and are considered as part of the evidence for intelligence in anatomically modern human beings. The simplest solution would be to propose that some modern humans lived nearby and the Hobbits copied their stone technology. Or that the Hobbits themselves also 'invented' sophisticated late Paleolithic technology. There ought to be some **burden of proof** for those who say that the stone work was not the work of the Hobbits.

In the PERSPECTIVES section of *SCIENCE* (vol. 306, 17 December 2004) we find Jared Diamond, a famous geographer and bird watcher from UCLA, talking about "The Astonishing Micropygmies".(pages 2047-8) and raising some very interesting questions. He finally reached the same conclusion that David Pilbeam and Milford Wolpoff confessed to – he could not explain the Hobbit problem.—but along the way he did examine many aspects of the puzzling situation, just as they had done. [Indeed his efforts did stimulate me to think of yet another possible solution –ED]

Diamond examined the sea levels during various time periods on both sides of Wallace's Line and concluded that walking from Asia to Flores via Java had never been possible for animals or hominins. One had to travel by boat or raft or floating log (or small tree or big shrub) or one had to fly or swim. The greatest gaps of all were between the Indonesian islands and either Australia or New Guinea, even when their continental shelves extended quite a ways to the west. Nevertheless, some placental mammals had crossed Wallace's Line at various times in the past, as of course the Alpha mammal of all the placentals – *Homo sapiens* – had crossed over and even reached New Guinea and Australia. The biggest gap even at lowest water was still around 52 miles (87 km) and very few placental mammals ever crossed it. Diamond added the new slant that no one could see across the Timor-Australia gap, for example, and thus not easily set out for the other side when he knew not that there was another side. (Columbus' old problem.) In the Indonesian realm on the other hand one could see across most of the gaps and thus make the effort. [And we may add that the probabilities of reaching other islands fortuitously while floating or drifting were much greater in the Indonesian realm –ED].

What struck Diamond the hardest was the realization that Homo sapiens, whether proto-Australian or proto-Papuan, had gone through – must have gone through – the Indonesian realm whilst on his journey from Asia to become the Australians and Papuans.How could they have missed Flores, a fairly big island, when they crossed from Bali to Timor? How could they not have known the 'micropygmies' when they crossed the Indonesian realm more than 28,000 years before the last of the Hobbits allegedly expired on Flores? Diamond assumes as a nearly definite fact that Homo sapiens crossed Indonesia at least 46,000 years ago on his way to Papua and/or Australia.

Resting on the assumption of non-credibility, Diamond spends some time speculating on the quality of the interaction of the modern men and the Hobbits, including conflict and/or intermarriage. In an interesting discussion of sexual interaction between humans and chimpanzees, he concludes that, like chimpanzees, the Hobbits would have been too ugly and too dangerous or tricky to copulate with. He also concludes that a symbiotic relationship between modern humans and the Hobbits, similar to that pertaining between pygmies and villagers in the Congo, would have been unlikely because both parties would have been hunter-gatherers and therefore competitors!

The last is a surprising statement from an expert on Australasia and its geography; he seems ethnographically challenged. While there are examples of hunter-gatherer co-existences in Africa, primarily Khoisan, there are more in the Americas. But New Guinea and Oceania have few because agriculture is quite old in New Guinea. Still in nearby Australia we find **hundreds** of hunter-gatherer peoples co-existing – peacefully or not so peacefully – all over the whole continent. We will have to ask Geoff O'Grady if he knows of **any** native Australian farmers (slash & burn or otherwise). I know of none.

Neither old Papuan agriculture nor Malayo-Polynesian agriculture ever reached Australia or Tasmania; or they failed to remain if they did reach there.

Diamond had one more proposal which is entirely legitimate in archeological terms because such things do happen from time to time, the most dramatic being the re-analysis of Choukoutien and Qafzeh/Skhul by Bar-Yosef. It would be a lot easier to solve the problem of the micropygmies, thought Diamond, if their dates were more in line with their classification as *Homo erectus* descendants. Could the dates taken at the Liang Bua site be mistaken? Was it really 18,000 BP or was it much older, like closer to 80 or 90 kya? Determining the answer to that question is a technical matter beyond our capabilities as non-archeologists. Since, however, the site was described as kind of 'squishy' [not the excavators' terms –ED], perhaps the dates were erroneous.

Since the Hobbit problem has been opened up to wider speculation by Diamond's efforts, we can propose two solutions which might work. The first is that the Hobbits were not directly descended from *Homo erectus*, even though there had been indirect evidence of such back in 800 kya. Both Pilbeam and Wolpoff had thought it possible that *Homo floresiensis* was a 'dwarfed version of a post-erectus/pre-sapiens' line (Pilbeam), a line distinct from other erectus descendants, or closer to *Homo habilis* or even the Austropithecines (Wolpoff). Pilbeam had thought the last alternative very unlikely.

The second solution actually has been proposed by one of the quarreling Australian archeologists – I am loathe to name them. The Hobbits were actually *Homo sapiens*. Indeed were this solution true it would suggest that modern man got to Flores, and therefore had earlier left Africa, as early as 94,000 years ago. Between that date and some time earlier than 18,000 these modern humans had become severely reduced in size. Maybe prehistorians and geographers have overlooked the Rampasasan villagers who claim partial descent from the Hobbits. By most measurements some of the villagers at least would qualify for the label 'Pygmy'. Even granting that Southeast Asia generally seems to exert selective pressures towards smaller size and 'Negrito' [read 'Pygmy' –ED] populations spring up frequently, still no one has ever been observed – except young children – living to adulthood in such a dwarfed state. Perhaps natural selection worked more rapidly and drastically because these hunter-gatherers were in fact PREY, while the big lizards were the predators. And so could anatomically modern men have been the predators, despite Diamond's rejection of that alternative!

¿Quien sabe?

One of the key elements of the main narrative we all are working on is the set of expectations we have for Southeast Asia, including the Austronesian realm and much of India. This general belief is much vaguer than the one we have for Europe, Central Asia, and the Middle East. There we expect that modern men met and perhaps competed with Neanderthals. In the east there is only a murky expectation that later day *Homo erectus* populations will meet the modern men and then basically vanish. We know quite a bit about their ancestors but precious little about the contemporaries of our modern folk. And in a place heavy in forests and islands there is a distinct possibility that we may find the old natives to be quite small. For causing us to begin to realize that, we may thank the excavators of Liang Bua.

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The Great Archeological Debate on Human Origins

The Great Debate: "Human Culture: When Did it Begin?"

The Setting: In the month of May 2003 in Washington, D.C. at the Smithsonian Institution under the auspices of the National Academy of Sciences. In attendance to record and report the debate for ASLIP were Daniel McCall and Harold Fleming. The debate was specifically arranged by, and introduced by, the Smithsonian Associates. The Smithsonian Associate speaks first:

We welcome you to our program this evening on "Human Culture: When Did it Begin?" There will be opportunities for audience questions at the end of the program. If you have a question, if you would move to the aisle in front of one of the microphones and field them from there. This evening as you know is a discussion debate. Our moderator tonight will be Dr. Bar-Yosef from Harvard and our speakers and discussants will be Dr. Alison Brooks and Dr. Richard Klein. At this point I will introduce the three of them and after that Dr. Bar-Yosef will open with some remarks and then we will have brief presentations from both of our other two speakers.

Dr. Alison Brooks is Professor of Anthropology at George Washington University where she has been since 1988. She is also Director of George Washington University's Geo-Biology Program, a researcher in anthropology for the Smithsonian Institution's National Museum of History, and a visiting scientist at the Carnegie Institute of Washington's Geo-Physical Laboratory. Dr.Brooks earned all of her degrees at Harvard University. She is a Fellow of the American Academy of Arts and Sciences and holds memberships in a number of professional organizations. Like all our speakers tonight she is widely published in a variety of journals and books.

Dr. Richard Klein is Professor of Anthropological Science at Stanford University where he has been since 1993. Previously he taught at the University of Chicago for 20 years and where he also earned his PhD. His latest book – *The Human Career* – summarizes the existing anatomical and archeological data on human origins and employs these data in a comprehensive evaluation of theories concerning the biological and cultural origins of our species. Dr. Klein serves on various editorial boards and has been a long time editor of the Journal of Archeological Science.

Dr. Ofer Bar-Yosef is the McCurdy Professor of Prehistoric Archeology at Harvard University and is Curator of Paleolithic Archeology in Peabody Museum. He came to Harvard from Hebrew University in Jerusalem where he was Professor of Prehistoric Archeology in the Institute of Archeology and where he earlier earned his PhD. His field work has focused on sites in the Middle East. Currently he directs excavations at Hayonim Cave in Israel which has yielded many important discoveries.

So to begin the program as I mentioned we will first have a brief presentation from Dr. Bar-Yosef. Please welcome all of our speakers.

!! APPLAUSE !!

Hereinafter the debate was recorded as nearly verbatim as it was possible, based on two of us attending the debate in person and a tape recording of the debate given to us by the Smithsonian Associates whom we thank. We missed very little of the debaters' spoken words. However, a great deal of information was presented by the debaters but proved impossible to tape record or adequately transcribe in our written notes. All speakers used a number of diagrams, maps, pictures, and other illustrated means to make their points, all of which benefitted the audience clearly. None of that rich material is included in our report, although we have made an effort to get copies of some diagrams. We regret the lacks. Editor

Obviously the deficits in the recording of the debate could have been remedied by cooperation from the debaters and moderator; they could have sent us diagrams, maps, etc., as enhancements to the tape-recorded material. They could have read the transcripts which we offered to send them and changed some misrepresentations of their ideas.. From May 2003 onwards one ignored us completely, another said he was too busy to do anything, and the third scolded me lightly for sending snail-mail instead of e-mail but sent no corrections of his short text.

Since we were trying for MT-Treatment of the Great Debate – which was after all about one of ASLIP' central concerns --, we sent copies of the verbatim reports (or offered to) to 4 Africanist archeologists, 10 paleoanthropologists, 10 geneticists and a few others and asked for their comments. Out of about 30 requests we got 3 comments. Although the three were valuable, the overall response was very disappointing, casting serious doubt on the viability of the whole notion of MT-Treatment. When the issues are trivial, we don't expect large responses but when the issues are serious, well, we expect…

Arguably the primary cause of this scholarly debacle was the incompetence of the editor, Harold Fleming, who took ages to contact people, who used the postal service instead of the internet, and 'blew' the crucial period, the three months right after the debate, when contact with the debaters would have been most helpful.

But the final cause or trigger of said debacle was the current culture of legal fears among editors and publishers in North America. Because the debaters would not answer me, would not read the transcripts of their own remarks and would indeed not cooperate at all, then it became impossible to publish our verbatim accounts of their speeches. Why impossible? Because – I was advised by experts – we **could be sued** for publishing those transcripts if the debaters did not agree that they had said such things as we reported. Even though Dan and I had heard them say these things publicly, and the Smithsonian Associates had recorded distinctly what they said, still the debaters had to agree that they had said such things. Holy cow! How do newspapers stay in business with such fears!

These are the reasons, therefore, that we cannot report the great debate after all. At least not in the kind of detail that would have satisfied our members. What we can do, and will do straight away, is to give a precis or rough and ready summary of the main points of the debate. This will be followed by the three comments from our reviewers and a final critique by the editor. There were serious issues in the debate, including one which none of the three archeologists in the debate gave voice to.
But first a little background:

Editor's Prefatory Remarks:

One of the main interests of the Association for the Study of Language In Prehistory has been to date and locate the origins of modern man, *Homo sapiens* or *Homo loquax*. It has always been possible to propose in theory at least that there was no one location, nor any necessary chronology, that mankind simply occupied the bulk of the Old World and had done so for hundreds of thousands of years and had simply evolved into modern mankind on the spot or in situ. Human language or modern human language had evolved along with, and was part of, the general evolution into modernity. In the classic formulae of distribution theory, human language was an innovation of *Homo sapiens* along with other innovations, rather than a product of diffusion. Modern mankind invented language everywhere they needed it. There might be, but there might not be, a common human ancestral language, a so-called proto-human, which underlay the known languages of the world. Theoretical linguists like Chomsky had proposed a Universal Grammar which preceded actual or realized grammar but it was not necessarily an ancestral grammar; it was more a conception of a grammar which underlies all others. Since it is said to be present in the psyches of children and thus made possible their acquisition of whatever language their parents were speaking, it was as much a piece of common or universal psychology as an aspect of any putative proto-language. In short human language was a species characteristic with the genetic attribute of being embedded as a potentiality or executive director in the psyches of human children.

In the mid 1980s there came a series of powerful hypotheses that challenged the status quo, the stable uncertainy and general lack of clarity with its attendant lack of interest in deep human prehistory or the origins of modern mankind and human language. Within a few years of each other Rebecca Cann and colleagues had proposed a strong version of an old idea (probably Darwin's?) – that modern human beings were genetically derived from Africa. Paleoanthropologists like Chris Stringer proposed that the fossil record showed that *Homo sapiens* had originated in Africa and spread out from there and perhaps did it not more than 100 kya or so. Neuro-linguists like Philip Lieberman challenged Chomskyite theory about the acquisition of language in children and also specifically denied that *Homo sapiens neanderthalensis* or (simply) *Neanderthal* could speak any modern human language, thus implicitly pruning the fossil record of possible cases of language development and focusing attention by default on the early African and Levantine fossils of *Homo sapiens* type.

(Hereafter we will ignore the many discussions, conferences, and writings of what I call the "Hardware Approach" to human language prehistory. It is simply beyond the scope of this chapter. Roughly put, we are seeking the 'narrative' of human prehistory, whilst the hardware people are seeking an 'explanation'. We think they cannot do that adequately without the narrative. Unfortunately, many of the brightest linguists find the creation of the narrative terribly boring.)

Archeologists like Ofer Bar-Yosef pursued more satisfactory dating techniques and produced a startling break-through by dating the clearly modern human fossils at Qafzeh and elsewhere in Israel to 90,000-110,000 BP, thus predating the arrival of Neanderthals in the Levant and offering dates of modern humans ostensibly leaving Africa in keeping with Chris Stringer's 'Out of Africa' proposals. And finally significant movement in deeper older linguistic taxonomies began to emerge from the dynamic young Russian linguists around Moscow. The older hypothesis of Nostratic was pumped up and bruited about, while an exciting marriage of Caucasic languages with Sino-Tibetan and Na-Dene was proposed by Nicolaev and Starostin. In Boston Alan Bomhard produced another version of Nostratic which also provoked interest. Then Joseph Greenberg came forth with the Amerind hypothesis which was an equally great or provocative advance; it ignited the main mass of Americanist linguists into something like hysterical opposition and led to a fire which virtually burned down the field of historical linguistics in the United States.

This August (2005) sees the 19th anniversary of the first meeting of the young Russian linguists in Moscow with the international community of scholars attending an Ethiopian studies conference, including myself. The intense interactions of those passionate young linguists with the Africanist linguists, usually moderately venturesome, and the more cautious Semiticists resembled a flow of hot lava entering the cool sea and led eventually (November) to the first Newsletter of MOTHER TONGUE and founding of ASLIP.

This all happened in the context of the rising excitement about human origins discussed earlier. From the very first we reached out to paleoanthropologists, archeologists, geneticists and linguists in general. You might say that we long rangers (Roger Wescott's term) hitch hiked a ride on the emerging paradigm. Yes, but no one really owned said paradigm in the mid 1980s and the potential contribution of linguists to the increasingly holistic paradigm was noted and appreciated. It is said in Anglo-American folk wisdom that "a rolling stone gathers no moss." Advice designed to get youngsters to settle down, no doubt. But consider what you get when you roll a round snowball down a snow-covered hill; it gets bigger and bigger! For a while our joining with others in the new paradigm, or more accurately a search for a new paradigm, was like a rolling snowball. But by the mid-1990s our sector of the emerging paradigm was losing traction, being slowed down and reduced in size because we were rolling on dry grass. This can be attributed in part to our enthusiastic addition of Joseph Greenberg and his associates, especially Merritt Ruhlen, to our roster. The matter is complicated.

Yes, an alliance with Greenberg brought the Amerind hypothesis, as well as his immense prestige among anthropologists, especially the very capable prehistorians, Steve Zegura and Christy Turner. But Greenberg's coat tails were on fire. A horde of outraged Americanists were attempting to tar and feather him, or at least drive him out of town. His sin was that he was violating THEIR paradigm of scrupulous methodology, painstaking field research, meticulous reconstruction, and hyper-caution. Linguists have often been an anal bunch but these angry Americanists were carrying that tendency as far as it would go. Nothing would do except perfection but the perfect thing had to be done by one of their own, for they were very much a social.group. Rigorously rectal groupies.

Here we are then. Discussion of human origins has proceeded with less and less participation by representatives of the field of Linguistics. With the death of Greenberg it may have been felt that we lacked people to represent his views, which had increasingly come to be seen as less relevant to continuing discussions because his bold taxonomic hypotheses were on record but nothing new was likely to be added to them. The linguistic contribution to the discussion was doubtless seen to be **moribund**. At least that is one conclusion that one might draw from the great debate on human origins – the burden of this report.

In general what did the debaters argue about during the Great Debate ?

First, the primary focus of the debate was on the theses proposed by Richard Klein in his book *The Human Career* and other recent writings. Basically Klein's presentation at the debate was summarizing his theses and adding new material as wanted or needed. Alison Brooks was there primarily as an Africanist archeologist with the necessary expertise to challenge or agree with or modify Klein's theses.

Klein proposed or proposes that Homo sapiens was not fully developed mentally (cognitively) when he appeared in the Levant around 100,000 BP. In other words we should regard the fossil Palestinians of that date as 'archaic', not yet up to modern man in brain power. (He didn't use the term 'brain power') And as an archaic sapiens he was not able to produce the sophisticated technology, especially tools, or art or advanced social structure, or (dare we say) language, that his later sapiens followers were able to.

Continuing, Klein argued that Homo sapiens of 40,000 BP, give or take maybe 10,000 years but with a cut off point of say 60,000 BP., was fully modern man and had the full range of basic modern mental traits which the archaic fossil Israelis were lacking.. Rather clearly identifying Homo sapiens culture of, say, 45,000 BP with that of Europe. Klein compared the splendid cave art and other art of Cro-Magnon and others with what the world had seen before and found it to be evidence of cultural advancement and higher intelligence. Almost unspecified, but known was his belief or agreement that these smart Europeans were from Africa, that they had developed in Africa and had gone to Europe by way of the Near East. He proposed that the whole movement be called the 'Aurignacians'.

Seeking the cause or causes of such important cultural and/or mental changes in Homo sapiens, Klein found it/them in **brain development**, in increased mental ability, hence my reason to call them 'smart'. They were the smartest of the Hominids, the smartest who had ever lived, as measured only by their production of art, tools and social structure. The smartness or increased productivity might have been explained some other way, but Klein argued strongly that the smartness was due to **genetic changes**, shared within the migrant population of these folk. Listening carefully, I do not recall hearing the word language used as the name of a causal factor but that is misleading. Klein seemed keenly aware of the influence of culture on apparent culture. Although hunter-gatherers are supposed to be primitive vis-à-vis high tech cultures. Klein made interesting small factual comments about hunter-gatherers potentially becoming computer programmers. The culture does not necessarily reflect the intelligence of its bearers adequately can be illustrated by three examples. Two are along the lines of Klein's point while the third shows how cultures encourage cleverness in different domains

a) During the time of Queen Victoria, perhaps contemporaneously with evolving notions of European racial superiority in intelligence, it was reported in the British press that – how very shocking! – a group of Australian aborigines from the southeastern part of Australia had formed a cricket team and had recently defeated the best team Britain had to offer. Yet another group of aborigines had formed a chess team which was doing well in competition. (I am not positive it was actually chess but it was a game involving skill.)

b) Eric ten Raa, expert ethnologist on the (Khoisan-speaking) Sandawe, used to beguile people with his tales of the Sandawe navy. (These too are hunter-gatherers.) It seems a group of Sandawe had traveled with a British official (in what was then called Tanganyika) down to the Indian Ocean port of Zanzibar (or Dar es Salaam). They went just for the hell of it. There, to their amazement, they observed sailing vessels, especially Arab dhows. So they acquired a vessel (maybe a dhow), learned to sail it, fitted it out, and sailed away round the Horn of Africa, up the Red Sea, thru the Suez Canal, across the Mediterranean, up the Atlantic and landed in England. Shortly thereafter, they sailed back the way they had come but decided to keep going and so finally got to Australia. Poor primitive hunter-gatherers indeed!

c) The Amharas and their traditional enemies the Oromo, residents of central Ethiopia, are normally farmers or laborers if they have migrated to the city. They are clever people, witty, sarcastic or satyrical on demand, wry in humor, and highly adept at mocking their betters but escaping punishment. They enjoy twisting and turning their complicated language, much as Poles do; they are great story tellers. On an ordinary human level they strike one as intelligent people. Therefore an Ethiopian government agency, the highway department, asked me to produce an intelligence test suitable for screening applicants for highway jobs like bull-dozer operator, truck driver and the like. So I selected an American children's game of fitting keys into locks and putting chimneys of appropriate sizes and colors onto various houses, so as to avoid a paper and pencil test which most would fail. Our test was highly visual and hands-on. We gave the test to the secretarial and engineering staffs of the highway department. They all passed easily and enjoyed playing the 'children's game. Yes, they all spoke English.

Then I gave the test to a large group of applicants, basically laborers, who wanted to go on to higher things, i.e., make more money. After much groaning and cursing and insults shot at me, the one who was torturing them, all of them, the whole lot, FLUNKED THE TEST. They all failed utterly. No one even got one key in a lock! How could this be? When I asked some of them what had happened, they replied that nothing in their lives had prepared them for this ordeal! They didn't know what to do or how to proceed. The highway department and I agreed that the test was **culture bound,** so we threw it out. Yes, it was a timed test which probably made a difference. No, they did not speak English.

Part of Klein's proposed genetic changes which were so important was the 'language gene' (Foxp2) which Klein proposed occurred at the same time period. It was actually difficult to sort out his theses because the language gene clearly was part of the genetic change but at no point did he propose that the acquisition of language had anything to do with the advancement of the 'Aurignacians'. His argument gets very slippery at this point but it seems to be a firm conclusion that a language gene was part of his genetic package but that the presence of language as a cultural or social factor was not mentioned.

Brooks' challenged Klein's main argument on three bases, (1) that there was a language gene or that it showed up around 40 or 50 millennia ago, (2) that the important or crucial genetic changes or the cultural changes showed up, 'all at once' so to speak, in that time period, and (3) that there had not been a gradual smartening up in African cultures before 40 or 50 millennia ago. She spent a considerable amount of time discussing the salient East African cultures going back to 70 or so millennia ago who showed increasing complexity and technological skill in their stone tools. And as is well known, she cited the art work of a South African culture (Blumbas) of earlier times

(70,000) as evidence that Africans could do significant art work before they became Aurignacian. She also advanced the interesting idea that the Aurignacians had actually gotten smarter after they settled in Europe because they commenced to eat **fish** which produced added brain nourishment. (We have forwarded that argument to the Gloucester Chamber of Commerce.)

Klein countered with a number of arguments, such as (1) the South African so-called art was highly unusual, not at all characteristic of its area or time period, (2) that much of eastern and southern Africa was barely habitable for thousands of years because of aridity with many areas being in fact abandoned by human cultures, and (3) Aurignacian art in Europe was distinctly more complex, 'better'art if you will. Brooks countered with long vivid depictions of East African tool kits back before 70,000 BP, especially fishing/harpooning sites she had excavated herself, which showed considerable sophistication. Her discussion of atlatls or spear-throwers was particularly to the point.

Thus endeth the summary or precis of the Great Debate. The commenters were shown the whole transcript to which they reacted. The first is **Daniel McCall** reacting to Klein's emphasis on art, especially European cave painting. Dan is a four-fields anthropologist, focused on ethnology and culture history; he reckons Alfred Kroeber as one of the most important influences on his thinking. His field work was done in West Africa. He has written about and taught African art for many years. Now Emeritus Professor of Anthropology at the African Studies Center of Boston University he lives in Boston.

ART OR ITS ABSENCE IN EARLY ARCHEOLOGICAL SITES

The presence of wall paintings of great age in caves is sometimes taken to be an indication of early evolution of artistic capabilities of the human group responsible for the execution of the paintings. Does the absence of such art at other archeological sites mean that the people who left those remains are biologically incapable of art?

Let us begin with a modern case. Some years ago I visited an exhibit in the Petit Orangerie Museum in Paris that was presented as 'Polish Art'. I was surprised to find that the paintings, usually portraits of important people of earlier generations, or family groups, were typically painted by German, Italian, or French artists, There may have been a Polish painting in there somewhere, though I did not notice any, but the overwhelming impression given by the collected works was that painting was not a Polish activity. This was not to be taken as an indication that individuals of Polish descent were not capable of painting, but rather it demonstrated that this kind of painting was not a prominent part of Polish culture.

This was another example that the arts (painting, sculpting, music, and literature) are developed to different extent in different cultures. Italians and Germans have produced most of Europe's music (at least the kind that is played in concert halls), with French and Russians contributing to a lesser extent and England hardly at all. England can hold its own with any other European counterpart (wherein there is great variability in quantity and quality). The creative energies of each cultural community seem to be directed into a limited sequence of the whole scope of the arts. [Not forgetting to add dance and theatre to the list – ED]

Archeologists can find some of the material remains of certain arts, but for others such as music and oral literature there is no basis of knowing how much or how little this genre of creativity flourished. Musical instruments may give some indication if they are found but the survivability rate of the materials of which they were constructed makes a negative decision uncertain.

Culture can be diffused. Japan did not have pianos before opening up to the West; now they are part of modern Japan's culture alongside instruments of greater age in Japan's musical tradition. Implements less complex than pianos were always readily adopted. Techniques to prepare brushes, or crayons; and mix pigments with fats or other vehicles, once worked out in one community could be borrowed by their neighbors.

"Rock art" (painting or incising on rock surfaces) is found virtually worldwide. A few of these are dated to several tens of thousands of years, but most do not go back beyond the metal ages, when tools to cut into rock became available, though stone can work on stone, and some Amerindian and Polynesian engravings are of stone age origin.

My conclusion is that the inclination to create art in several genres is universal in human populations. Individual talents vary, but every community of any size will have some tradition in one or more of the arts, which ones predominate depend on historical factors.

[In 1955 and 1956 I witnessed a significant cultural change in New Haven, Conn. Whereas graduate students and their wives would gather to sing songs of Whiffenpoof style in the collective back yard and drink beer in 1955, someone brought a **guitar** in 1956 and changed the whole scene. How? First, the music changed to suit what the guitarist could play (blue grass, country and such), 2nd the singers more and more became listeners, while the guitarist became a performer, and 3rd a great student singing tradition was replaced by increasingly commercialized music. Just because he brought his guitar! – ED]

Our second commenter is **Henry Harpending**, formerly at Pennsylvania State University and now at the University of Utah in Salt Lake City, Utah. Professor Harpending is a population geneticist and one already well known to members of ASLIP for his earlier work on the theory of human dispersals out of Africa. His notion of 'nesting areas' has influenced the thinking of all of us.

Henry Harpending's comment took the form of a personal communication over the internet. So, quoting him directly, here is his comment.

Here is my brief commentary on the debate. (Best, Henry)

There are several specific comments that I can make from the viewpoint of a geneticist but I have no competence at all to evaluate the archaeological evidence from Africa before 40,000 ya, which is central to the debate.

Both parties acknowledge genetic support for the idea that AMH [Anatomically Modern Humans –ED] came storming out of Africa replacing indigenes. But it has become clear from studies of the nuclear genome that such a dramatic replacement event did not happen. The model only received strong support from mitochondrial DNA, and it now is clear that mitochondrial DNA is a far outlier from other genetic systems. Mitochondrial DNA almost certainly was replaced but the nuclear genome almost certainly was not. The clearest statement of the implications of evidence from the nuclear genome is in Eswaran's contribution to Pearson et al. 2003, especially his online material. Most current literature about this issue reports signs of expansion in the nuclear genome but these all seem to reflect post-Pleisocene population growth.

Klein's position is that something new in the genome showed up ca 45,000 ya, leading to the exodus of AMH from Africa. But some simple genetic change could have spread rapidly through the world population of archaic humans, if they were all conspecifics, without any accompanying morphological change at all. Klein therefore needs the African AMH to be a new species, but the genetic evidence today denies the older picture of a dramatic expansion of a new species.

It is easy for me to say what the genetic evidence does not support but it is not so easy to say what it does support. There are two rather solid findings from studying the nuclear genome, findings that are apparently blatantly contradictory. They are (1) the amount of genetic diversity in our species is that of a species that has 10,000 breeding individuals and (2) there is no evidence of any Pleistocene population explosion from the nuclear genome. These are contradictory because (1) implies our descent from a small focused founding population, but such a history should leave a clear strong signal of expansion that isn't there (2). The only models so far that can reconcile these data are (1) a model that proposes high levels of selection on the genome (Harpending and Rogers 2000) and (2) a model of the spread of an advantageous complex genotype (Eswaran 2002).

In my opinion a difficulty with Klein's position is Australia. Ofer mentioned the problem [in the verbatim transcript –ED] but neither Klein nor Brooks took it up. Humans showed up in Australia at approximately the same time they invaded Europe and not earlier (O'Connell and Allen 2004) bearing Neanderthal grade technology. If the bones, beads, and beautiful things of the European Upper Paleolithic mark something new in the human genome, it wasn't in the genome of the folks who went to Australia. Is the aboriginal population of Australia really a late arrival, for example with the dingo? Were the Kow Swamp people a different species?

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In a subsequent e-mail Henry clarified his point about Australia.

> The gist of the comment is that it is difficult to accept Klein's whole thesis because it does not fit all parts of the world. Thus, if what Klein is saying is that the first 'real' human diaspora from Africa came circa 45,000 BP, instead of 100,000 BP, then one would expect considerable similarity between/among cultures found in various parts of the world during the same epoch. Thus, the Australian evidence of around 45,000 BP does not conform to the Aurignacian culture patterns. Since Australia is farther than Europe and more difficult to reach from Africa , then, if anything, the Australian settlement ought to have left Africa even earlier. If the Australians are derived from the same common modern Homo sapiens who allegedly left Africa around 45,000 years agi, then why are they not like the Aurignacians. Or why are the Aurignacians not like the Australians? Or why not derive the Australians from the 'archaic' Levantine fossils?

A third and full-throated comment comes from Christy G. Turner, II, Regents' Professor of Anthropology, Arizona State University, Tempe, Arizona.. Professor Turner should be very familiar to Long Rangers because of his taxonomic work in the western Pacific, eastern Asia, and the Americas, based on very careful dental measurements and comparisons. Hereinafter is Christy Turner's comment.

The following commentary is based on my reading of a transcript provided by Harold Fleming on the debate between Drs. Richard Klein and Alison Brooks, held in May 2003, at the Smithsonian Institution, Washington, D.C. My text was completed in May, 2004. Minor corrections and the Macauley et al (2005) citation were added in May, 2005.

> The understanding of modern human origins is thankfully moving beyond the log-jammed debate of single versus multiple origins. Single origin theorists are winning because their evidence is much greater, more varied, more parsimonious, more redundant, and more compelling than what has been offered for multiple origins (Stringer and McKie 1996; Tattersall and Schwartz 2000). Biologically, modern human groups are almost identical in our holdings of genes, skeletal and dental features, many other structures, and in a little-noted trait that I have been casually observing during my years of world-travel – an identical bulbous-ended down-turned second toe that I call "world toe." It seems to be invariant. I doubt very much if its universality has much to do with strong selection pressure as will be argued by the discussants for low variability. Sewell Wright (1956) proposed many years ago that low or even the absence of variability could arise by chance alone. As interesting as world toe is, it is not as useful for the present discussion as is dental morphology, about which I would like to say a few words as it relates to the Klein-Brooks debate.
>
> Variation in modern human dental morphology seems to have begun sometime around 50,000 years ago on the grounds that Neanderthal and Cro-Magnon teeth are very much **unalike** (Bailey and Turner 1999). On the other hand, Cro-Magnon people had teeth very similar to those of modern Europeans.

Modern European teeth, in turn, are similar to those of India, less similar to African and Southeast Asian , and quite dissimilar to Northast Asian and Native American teeth (Turner 1995). Some Sri Lankan teeth of 20,000 years ago are much like those of Southeast Asia (Hawkey 1998). These and several other such relationships are largely what would be expected on the basis of geographic distance, language family relationships, other biological data, and a considerable amount of archaeological information. Dentally, Southeast Asia (which presumably included late Pleistocene eastern India) was the center of the world, that is, teeth here are the least divergent with all other worldwide populations. Said another way, Southeast Asian teeth are about as similar with Europeans as they are with Chinese or Africans, whereas African teeth are fairly similar to teeth of peoples inhabiting the southern hemisphere of the Old World and Oceania, and very dissimilar with Chinese, Japanese, and Native Americans. How this Southeast Asian centrality came to be is most unclear. Both a hub-and-spoke and a cross-roads model are possible. There is insufficient subfossil human remains to help decide one way or the other. The former model envisions the world's populations originating from a group whose teeth were like those of Southeast Asians. The latter model envisions admixture between an existing archaic East Asian population and migrants coming from somewhere else, presumbly Africa. Regardless of which is more likely, diachronic and synchronic dental analyses suggest that the migration route of modern humans out of Africa into Europe was not a straight forward northward advance, instead, a more eastward origin is hinted at.

Africans are dentally less divergent than Native Americans meaning that time and divergence are not strongly correlated, an assumption that underlies all of the genetic trees from which much of the out-of-Africa origin for modern humans is based. The Native American population is most likely only 13,000 years old, or if we extend their ancestry back into Siberia, their formative ancestry would likely go back less than 20,000 years ago. The age claim for modern Africans is, as the debaters suggest, at least 50,000 years. Natural selection was very unlikely to have had much effect on the evolution of the crown and root traits used in the above analyses (several are worn off before reproductive age). Most likely the variation has arisen because of genetic drift, population structure, and founder's effect. I will comment more on this later because to my way of thinking it bears on the views of both Klein and Brooks, namely, much more consideration needs to be given to the effects of chance in the modern human origin question.

The Klein-Brooks debate reminds me of a classroom event I participated in during my senior year of high school. That event was a formal debate about the importance of **nature** versus **nurture** in forming humans. The arguments went back and forth. Near the end of the hour I remarked that the answer was not one or the other, rather both are involved. And, if my memory is correct, I used language as the means to make my point – we all learn early in life to speak one or more languages because we have it in our nature to be able to do so. No other animals speak a language because they lack the genes that would enable them to do so, although chimpanzees are able to learn to communicate by sign language

(Gardner and Gardner 1994, and elsewhere). These "language" genes are clearly those involved in the morphogenesis of vocal anatomy, brain size, brain organization, postnatal growth rate, and several other considerations, not just the Foxp2 gene. In this context, Richard Klein seems to slightly favor the **nature** side, whereas Alison Brooks clearly favors **nurture**, although they both seem to appreciate the need for a combination of genes and culture to explain the origin of modern humans.

A second difference between Klein and Brooks that stands out is the former views the archaeological record at about 50,000 years ago as having had a significant biocultural fluorescence or behavioural saltation, if you will, a kind of Sewell Wright vision of how evolution can sometimes proceed. The latter has a much more gradualist, Darwinian, linear, or Marxist-Leninist view of "progress", as seen in her suggestion that it was a long period of technological improvements that led to larger population size, which in turn eventually caused modern humans to migrate out of Africa. Here, nothing is claimed for genetic changes. These views arise, to my way of thinking, from Klein's more bioarchaeological inclinationas judged from the debate and from his numerous publication, whereas Brooks approaches the matter from a more cultural archaeology viewpoint, judged herein and from her publication as well. Political correctness unfortunately may be involved here also because of the diversity and richness of the cultural inventory of the early modern humans in Europe in contrast to that known so far for Africa of the same or similar time. Both dating and taphonomy may be influencing the inter-continental differences.

It is important to note the difference in their acceptance of chronometrics for the archaeological record. Klein appears to be more skeptical of the reliability of various dates than does Brooks. This gives Klein more flexibility or fuzziness in his archaeological story, while it tends to commit Brooks to a more rigid date-cultural stage reconstruction. Klein's views are less neat than those of Brooks, and depending on how one sees the world either view could be the better approximation of the past. Time, space, and association are the three domains of archaeology. Klein and Brooks deal adequately with time problems, and more than adequately with space. It is the association domain, which involves taphonomy, that I worry most about.

As for taphonomy, in my experience, both stratigraphy and dating can be markedly influenced. For example, in the Aleutian Islands of Alaska there are no trees, so all the charcoal-based carbon 14 dates are derived from driftwood that required months to decades of drifting time to be beached and even re-beached eventually at or near an Aleut site. It was just this sort of non-community association that Yefremov (1940) was concerned about when he defined and coined the term **taphonomy**. Another example. For the last six years I and two Russian collegues have been studying cave and open archaeological and paleontological sites and faunal collections from these sites in Siberia (Turner, Ovodov, and Pavlova 2005). We quickly learned how ubiquitous the cave hyena was in the late Pleistocene of Siberia. In one archaeological cave site in the Altai region there was a marked stratigraphic reversal from expectation. Upper Paleolithic blade artifacts were found beneath Mousterian flake tools, which led to all kinds of ideas and speculation about the local culture and population history – all kinds of ideas except for giving thought to what the substantial presence of hyenas in the cave midden might have contributed to the stratigraphic reversal (Turner, Ovodov, and Pavlova 2001). The presence of modern hyenas in Africa and extinct hyenas in southern and eastern Europe are well known. Archaeologically-useful behavioral monographs have been published (see for example, Kruuk, 1972) detailing burrowing, scavenging from human settlements, and other taphonomically relevant behavior. Might some of the African chronology have been influenced by hyena digging, scavenging, and general messing up of ancient human camps? A few hyena "stomach bones" in a site should be a strong signal to proceed cautiously where dating and stratigraphic associations are concernd. This is a mnor technical point, but one that I felt should have been at least footnoted. I would like now to return to teeth to make another point.

At a conference in Japan in 1990 (Turner 1992a) I proposed a model for modern human origin and dispersal based on inter-group similarities and differences in dental morphology. This model was elaborated on in a second Japanese gathering in 1993 (Turner 1995). Basically, it was an argument against the multi-regional hypothesis, and an attempt to develop more facts and thoughts about the single origin, rapid replacement model. However, it differed from the "out of Africa" scenario by suggesting for a number of reasons that Australasia was the most recent general homeland out of which groups proceeded back into Africa, into Europe and Asia, out into Australmelanesia, and eventually into the New World, Micronesia, and Polynesia, seemingly in that order. I called the model "shifting continuity" in recognition that there had to be genetic continuity between archaic and modern humans, but that it need not necessarily be continuous in one geographic area. The interpretation was drawn from a very large data base that showed that the frequencies of crown and root morphological traits exhibited the least amount of divergence between Southeast Asians and all other modern groups, as described above. There was greater divergence beween Africans, Europeans, and Native Americans. That is, the average Mean Measure of Divergence was smallest in Southeast Asians and greatest in Native Americans. From a parsimony viewpoint, it would have been easier to evolve all the modern world's dental differences from the relatively simple and retained Southeast Asian Sundadont pattern, than it would have been to evolve them from one of the highly divergent patterns, that is African, European, and the Northeast Asian-Native American Sinodont pattern. Because Sundadonty is rather similar to the Australian dental pattern, it was suggested they shared a common pattern called Proto-Sundadonty that antedated the ancestral Aborigine arrival in Australia, perhaps 70,000 years ago in Sundaland or what Howells (1973) called "Old Melanesia". Thus, my dental scenario envisioned a watercraft-based expansion out of Southeast Asia, possibly triggered by the climatically-harmful Toba volcanic eruption in Sumatra at 73,500 years ago; if watercraft-based migrants could move eastward from Sundaland to Sahulland, crossing inter-island passes (Birdsell 1977, Bower 2003, Macintosh and Larnach 1976, Turner 1992b), then except for a much greater distance, I saw no reason why proto-Sundadonts could not have traveled eastward [sic –ED) along the Indian Ocean coast to northeast Africa. [We think he meant to say 'westward' – ED]

In general , the area where there is the greatest biological or cultural variation, is usually thought of as having been the oldest area for any trait or group. The very wide variety of riverine, coastal-hugging, and pelagic boats in Southeast Asia would make it a better area to begin a long-range water-craft-based migration than Africa. Recall, there is no evidence of human occupation for Madagascar until Sundadonts from Southeast Asia reached the island. Watercraft use in the late Pleistocene would have permitted relatively rapid travel, and would have been quite analogous to modern aircraft for the rapid spread of infectious diseases, my *deus ex machina* for replacing the archaic humans whom the Proto-Sundadonts encountered. Thus Brooks' archaeological focus on east Africa for modern human origins, especially Tanzania, has a loose but intriguing correspondence with the shifting continuity scenario. I do not know what sorts of stone tool correspondences she might require of the coastal-drifting Sundadonts, but as G. Pope (1989) has proposed, we might not expect there to be any since the stone tools of Southeast Asia were perhaps used mainly to prepare the secondary tools made of bamboo, a remarkable material much valued for weapons, containers, rafts, houses, and many other items. While the Southeast Asian stone tools are generally referred to as chopper-chopping tools, there are microblades and other small objects among these so-called Sumatraliths.

While genes can move without human migration, they are, at least today, more often carried by migrants. Certainly that is the case for the European-like dental pattern that shows up in the Sudan sometime after the end of the Pleistocene. Before then, Mesolithic teeth of Nubia are very similar to those of modern West Africans (Irish and Turner 1990, Turner and Markowitz 1990, Lipschultz and Turner, n.d.). Language also, probably moves like gene flow between adjacent groups giving rise to cline-like dialectic chains. But adjacent languages and language families with marked differences almost surely represent prehistoric migration events, i.e., Athapaskan in the American Southwest.

When first proposed, shifting continuity was generally ignored, considered too controversial (Clark 1994), or just plain wrong (Rightmire 1999). As originally proposed, it was based on several lines of evidence and a set of ideas. One of the latter was: continuity is like a null hypothesis, that is, it cannot be "proven", only discontinuity could. What might appear to be continuity could contain one or more hidden or difficult to identify discontinuities. Discontinuity was envisioned as the term is used in geology. On this basis, a fair case for discontinuity between Neanderthal and Cro-Magnon teeth could be made. Much less certain, but within reason was the likelihood for discontinuity in archaic and modern Africans, although the number of specimens was dangerously small to make such an inference. In Nubia at least, there seems to have been a marked discontinuity in human populations between 14,000 and perhaps 7,000 years ago caused by migration into north Africa from the Levant. As for Asia, no case could be made for Pleistocene discontinuity (see Pope 1992). Moreover six of S.M.Garn's (1965) nine geographic races can be traced directly or indirectly to Southeast Asia. Hence, Southeast Asia was suggested as an alternative homeland for modern humans with people spreading from there to Africa, Europe, and Oceania. More and more evidence indicates that this Asian origin inference lacked enough deep-time dental specimens of archaic humans to support such a far-reaching model despite so many other of its parts being quite reasonable. Some recent findings in Y chromsome research suggest that shifting continuity was not entirely wrong, and this evidence bears on another issue in the Klein-Brooks debate – namely, the all-too-common dismissal of Asia on the grounds that there is not enough information on the paleoanthropology of Asia, so only Eurafrica will be discussed as both Klein and Brooks do.

The Y chromosomal work by M.F.Hammer et al (1998:435) proposes that there was migration from Asia to Africa that contributed to a "large part of the paternal diversity in Africa". These workers estimated the return to Africa at roughly 30,000 years ago.

Very recently, an unusually supportive article for "shifting continuity" was published by Vincent Macauly et al (2005). In that article they propose that the initial movement of modern humans out of Africa followed a coastal route to India, then Southeast Asia, and then on to Australia. From somewhere in Australasia, moderns then backtracked northward to reach Europe. Boating is a key element in their reconstruction, just as it is in "shifting continuity", although they presumably envision the origin of the watercraft in Africa.

In sum, this was a very interesting debate that shows how difficult archaeological inference can be, that is, when two renowned specialists can look at the same evidence and come up with two quite different stories based on that evidence. My strongest criticism would be the much too casual way that both Klein and Brooks dismissed Asia in their search for an understanding of the how, when, and where for modern human origins. However, without a balanced treatment of human evolution in Asia, the out-of-Africa scenario could possibly become another just-so story like the 48 chromosome number that humans were supposed to have had only a few years ago.

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I must stand aside as editor and assume the role of fellow investigator. Like the others, debaters and commenters, I have interests, preoccupations and skills which can be brought to bear on the subject matter of the Great Debate. At this moment and on this subject I stop trying to be even-handed and neutral.

First, some criticism and some questions for the dominating thesis of the whole discussion, namely Klein's hypotheses.

1) The denigration of the intelligence of the archaic Palestinians at Qafzeh and elsewhere seems unfounded. Does the very similar Mousterian culture of their sites mean that they were Neanderthaloid cognitively? Does that make them not so smart? Or is it due to their larger teeth? Some modern groups, e.g., Australian aborigines, have larger teeth too. So what does that indicate?
2) Judging from Henry Harpending's characterization of the Australian tool kit, presumably from old archeological sites, may we not suggest that they were more akin to Qafzeh's 'Mousterian' than to Aurignacian? By Klein's way of reasoning does this not imply that the Australians were not fully modern behaviorally either and therefore not up to snuff cognitively? As we shall see, their descendants – Australia's aborigines – were clearly able to understand various aspects of British culture fully as well as the Brits themselves. It is not so clear that the reverse was true because understanding Australian **kinship systems** in their full complexity has been a problem for generations of anthropologists. This is not to deal, here at least, with the evidence that

indicates that Australasia was settled by modern men more likely to be offspring of Qafzeh types than Ausrignacians.

3) Brushing aside the amount of time the Aurignacians spent traveling from Africa to western Europe or even eastern Europe is tantamount to brushing aside an important variable for no good reason. Not even considering it is worse. When the Aurignacians left Africa, they were not producing great cave art there, so far as we know. Nowhere along their trail to Europe did they leave evidence of great cave art. So the only evidence of their artistic skill – at the level reached in France and Spain – was found in Europe. How long was their trail of settlement and movement and re-settlement from Africa to Europe? How long did it take them to make the journey? Perhaps 10,000 years? At least. Their first traces are probably in Egypt around 50,000, the Levant 45,000, and western Europe 35,000 to 40,000 at the earliest. (This all according to Ofer Bar-Yosef personal communication).
4) The Aurignacian route took them thousands of miles from their origins in eastern Africa – roughly somewhere between Khartoum and Zanzibar– down the Nile Valley, across the Levant and over to Iran, around the mountains and up into Central Asia, then across the prairies into eastern Europe, then through the mountains and valleys to the Atlantic fringe. (The route and the dates were laid out for me by Ofer Bar-Yosef in a personal communication several years ago.) Most of the non-African portions of the route would already be inhabited by strong vigorous local humans – the Neanderthals. Not necessarily demographically large, the Neanderthals may have resisted from time to time and probably can account for the long time it took the Aurignacians to finally reach western Europe. Climate conditions would also play a crucial role. One indication of their non-rapid advance would be the 10 to 15 millennia required to reach Georgia in the heart of the Caucasus Mts. around 1500 kilometers from Cairo.
5) Along part of their route, particularly the Arabian Peninsula and the Persian Gulf region and the Indus Valley, there is a fair chance the Aurignacians encountered other members of their **own species**, people with whom they could inter-breed and possibly converse and very likely do battle. The time depth of separation between the old humans and the new humans in, say, Oman or Sind would be comparable to that between modern peoples, say Apaches and Somalis – the product of 50,000 years of differentiation. The presence of the old humans in some parts of that region cannot be gainsaid and should not have been so easily forgotten by the debaters. Remember the archeological adage – the *absence of evidence is not evidence of absence.* More on these people below and in a later chapter.
6) Migratory peoples, and that is what we are talking about here, can change culturally, socially, physically, and linguistically in the course of their travels through time and space. It happened to the Scottish 'bordermen' who ended up as Texans by way of being Daniel Boones, after having been Catholic-suppressors in Ulster. And that was only a few hundred years. In a time period –by Christy Turner's dating of 13,000 BP – only a bit longer than the 'Aurignacian march' a migratory group from Siberia settled two continents,

adapted to many quite different ecosystems, and generated 500 or 600 different languages. There are some Amerind groups who look uncommonly wretched and ignorant (in lower South America) and those who look uncommonly smart and creative (in Meso-America). Which one reflects basic Amerind intelligence?

7) If Turner is certain that neither Klein nor Brooks took Southeast Asia seriously enough, Harpending and I are convinced that neither took Australia (or ill-defined Australasia) seriously at all. Africanists ignore the southwest Pacific at their peril because it is the world region most like Africa in its obvious time depth of human habitation and great diversity of human things. Klein, Brooks, and I are all Africanists with more than nodding acquaintances with the Middle East and Europe. We three all reflect the attention deficit disorder characteristic of most of prehistory; we focus too much on one part of the world. That is more Europe and the ancient Near East than anything else but consider how poorly even Indian prehistory is known, compared to Africa. China and Japan are exceptions to this rule, although most of the work is done by locals.
8) The people of Qafzeh and Skhul in modern Israel of 100 kya (more or less) were not taken seriously by Klein or he brushed aside their meaning as anatomically modern human beings. Klein's whole argument rested on the presumption that physical deviation from the fully developed humans of our era (the Upper Paleolithic to the present) represented deviation from the psychological and cultural abilities of modern human beings. In simpler terms it is saying that the archaic Levantines were not up to our level mentally because their heads were a little different. Yet the arguments about how smart Neanderthal was – with his big brain in an odd shaped head – are still not settled. Perhaps this is the Anatole France problem all over again? Brooks should have borne down hard on this point and not let Klein get away with this gross *non sequitur*. Perhaps she really agreed with him anyway. In any case Klein was ceded the grounds for maintaining that the Aurignacians of 50 kya more or less were the first fully modern human beings and the problem of explaining their cultural leap forward is THE problem of modern human prehistory. It is a little bit like saying that the French of the Enlightenment were the first civilized Europeans because **all** we know of ancient Europe is what Tacitus said about the Germanii.
9) Since Qafzeh and its kindred sites of 100 kya were the only ones of their type, are we justified in thinking that they were 'all there is to it' – the only occurrence of their type of people? Apparently our debaters think so. But, as Ofer Bar-Yosef has said elsewhere, people of Qafzeh type could have been living throughout much of northern Africa and the Near East and at times both earlier and later than 100,000 BP. I can remember once back in 1961 believing from linguistic and other evidence that there must have been domesticated cattle in the Sahara or areas bordering on it. My archeological adviser took a more cautious view, viz., no evidence for something means just and only that – you cannot say that something is present when evidence is lacking. The only possible position you can take is **agnostic** because you also

cannot say that the something was absent. Not too long afterwards another archeologist found cattle in the Sahara, dated to 5000 BC or thereabouts. That settled the matter. But the logic of our conversation remained valid. Let me utter a weak truism – there is an enormous amount of data for prehistory still lying around undiscovered by modern investigators. Everybody knows that but some people forget it.

10) Now we have an important addendum to section 9 (above). While we have only that one fossil in the Levant, still there is powerful backing for Ofer's point, if we consider the **cultural debris** instead of focusing solely on human fossils. There is an important section in Klein's book –The Human Career --, pages 394-424, previously unappreciated, which focused on the **'industries'** associated with the fossils or without fossils but in the same time period. Mousterian is **wide-spread** in northern and northeastern Africa (Sudan, Egypt, Libya, Tunisia, Algeria, and Morocco). As typologies, at least in Israel where both occur, what we might call African Mousterian can be distinguished from Neanderthalian Mousterian. [these are not Klein's terms – ED] So the conclusion would be that industries very similar to those of Qafzeh and Skhul were found widely in northern Africa at about the same general time period. From an apparent aberration -- modern man with a Neanderthal tool kit – we find that African modern man had his own tool kit whose relationship to Neanderthal's was not quite clear but certainly not borrowed or, if ultimately borrowed, taken up many millennia before. In addition one of Africa's mystery industries – **Aterian** with the tanged points – was typologically rather close to Mousterian and occupied much of the same territory, except that Aterian occupied more of the Sahara.
11) There are two archeological sites that suggest that modern men presumably of Qafzeh type were present over a wide area, Eritrea on the west and south of the Levant and India far to the east. The Eritrean site is fairly solid and actually older than the Qafzeh site. We will discuss it in the next chapter. The other is vague or nebulous because my knowledge of it was based on one personal communication from Alison Brooks who declined to elaborate on the details. Somewhere in India there is a site, dated to around 100 kya, with possibly Mousterian tools. It might even be a Neanderthal site! But as Qafzeh showed us, the Neanderthals do not own the Mousterian type of tools. Why they share it with the old humans is something no one has yet explained to me.
12) There is a fair chance that the old humans of the Levant were speakers of a very old language of human type and that language was related to or grew into the giant super-phylum of Australasia, culminating in native Australian languages. As we shall hear in Jonathon Morris' chapter on Trombetti similar things have been said in the past.

But now we must look at the other thing with which we strongly disagree – the question of LANGUAGE and what to make of it

1) Colleagues from Stanford or other parts of the West Coast had from time to time surprised me and others with something like the following statement:

**" Language is about 40,000 years old; it came out of Africa with the Aurignacians and spread around the world. This has been shown by Richard Klein of Stanford" **. Also surprising was the certitude of the dates. Since dating the birth and dispersal of human language was one of ASLIP's primary concerns, and had been for a full 19 years, I was amazed at that certitude and the lack of discussion that went with it. Somebody had been anointed and the discussion was finished —before it even began.

2) So part of the reason for traveling to Washington to hear the Great Debate was to hear the prehistory of language unfolded and discussed and to see what we made of all this.
   At the end of debate Dan McCall and I asked of each other: "Where was the part about language" Did you hear language even mentioned that whole time?" The answers said NO, NOT ME. I didn't hear it. After we finally got the whole transcript typed up, we saw that our answers were not quite accurate. There was the small section on the language gene, **Foxp2.** That did it for language. Now we were shocked.
3) What had gone wrong with Klein's argument? It was not hard to see what a difference it would have made to his argument if he had maintained that the advent of spoken human language, instead of some unsubstantiated and nebulous genetic change, would have transformed humanity mentally. Why ever would a scientist as obviously competent and bright as Klein throw away his best argument, and one that everyone expected him to make, for a bunch of airy fairy genetic factors?
4) Anyway, even if we replaced his genetic 'razzmatazz' with human language we still could not agree with the 40,000 year date. No, not at all. But the reasons were different. By now I had completed two hypotheses which disagreed with Klein's 'old' position, not because they were meant to show him mistaken on some points but rather to present competing hypotheses which could be tested along with his hypotheses.
5) Klein's Aurignacian hypothesis fit perfectly with my **Borean** hypothesis, as revised from its earlier presentation in MOTHER TONGUE, and gave it initial dates. Leaving Ethiopia around 50 kya, Borean eventually occupied Europe, the Middle East, Asia north of the Himalayas, the Americas in three movements, and later on China and Japan. The first or Amerind movement probably being attested to by an archeological site in northeastern Siberia in the high Arctic of 30,000 BP on the Yana River (Boston Globe 1/12/04, p.A2). Or altogether from north Kenya all the way up and around and down to Tierra del Fuego. Since this is an hypothesis, not God's Truth, it must be tested. And modified or thrown away if it is faulty.
6) More recently, I have proposed another older scheme, called the **Tropical hypothesis.** This would link every language in Southeast Asia (but minus Sino-Tibetan) and Australasia + Kusunda + Nihali together but with a tentative link to the big combined Niger-Congo cum Nilo-Saharan super-phylum in Africa. I am not the only one to have thought of this hypothesis, although we are essentially independent of each other. None of us can agree what to do with Dravidian which keeps jumping back and forth between

Nostratic, Nilo-Saharan and the Australasian group. Neither Afroasiatic nor Khoisan are in this tropical mega-phylum. Afroasiatic is the base family for the Borean mega-phylum, while Khoisan as yet lacks relatives but may possibly link up to Afroasiatic.

7) For purposes of this chapter's discussions, however, the importance of the Tropical hypothesis is that as the older –I would say much older and deeper – genetic grouping it bids fair to be associated with the Old Humans, the people of Qafzeh and all of that. The Old Humans may have shrunk from their rough contact with Neanderthals, moving south in Arabia and India and eastward otherwise. Until they reached the island world or Sundaland and the southwest Pacific. Contrary to Macauley et al they had no great need for boats, since during the crucial time depths they could walk from Aden to Sri Lanka or the Andaman Islands or to Java without any great trouble. Ice Age low water and all of that. Later on they could boat to Flores, or Timor, or New Guinea or Australia or the Melanesian islands.
8) Aside from Trombetti's distinct priority in time, something like the Tropical hypothesis ('Macro-Australiano') was proposed by Morris Swadesh in the 1960s and a young Russian linguist, Alexandra Aikhenwald, in the 1980s, Merritt Ruhlen and some of his colleagues in the 3rd millennium AD, and some at the Santa Fe Institute currently. Something very much like the Tropical hypothesis was given by Joseph Greenberg to Nicholas Wade of the New York Times (February 1, 2000, p.D1). The Greenberg comment was displayed in the form of a map which outlined the equivalent of **Borean** from Portugal to Patagonia by way of Novosibirsk. . However, Sino-Tibetan was not included and indeed painted a different color, while Caucasic languages were absent altogether. The Borean equivalent was dated to 40,000 years ago, while the **Tropical** equivalent was dated to "more than 40,000 years ago"; it did not include any African phyla.

The specifics of my Borean hypothesis may be unique, in the sense that no one has done it exactly that way before, but the essentials or most of the membership have been proposed many times before. Two scholars, Trombetti and Swadesh, proposed mostly the same thing in the early and middle 20th century. Trombetti called his 'Boreal', while Swadesh chose 'Vasco-Dene' [or Basque to Navaho –ED], but excluded Amerind. . The Nostratic hypothesis, while amended by Greenberg and others, was always tacitly assumed to be a genetic grouping, albeit not a valid taxon. Greenberg linked Eurasiatic and Amerind, even proposing a few etymologies, in the 1980s. Shevoroshkin linked some of North American Amerind to Nostratic in the 1980s. Starostin proposed that Sino-Caucasic was related to Nostratic (at a Moscow conference in 1984). Mukarovsky linked Basque to Berber, and later Afroasiatic in the 1980s. Blažek tied Elamitic to Afroasiatic and Dravidian, while Bomhard put Sumerian explicitly in Nostratic, as Greenberg joined Etruscan to Eurasiatic. Bengtson linked Basque to (North) Caucasic and Burushaski, while Kevin Tuite saw Burushaski as related to (South) Caucasic. John Colarusso related Indo-European to Caucasic, while John 'Ian' Catford explicitly joined Kartvelian to (North) Caucasic and even counted the cognates they held in common on a Swadesh list (6%). Several generations of linguists were, of course, raised on the standard linkage of North and South Caucasic. It was also fairly common for Dravidian to be linked to

Uralic, although Stephen Tyler's proposal is the only recent one I can remember. Recently, Ronald Thornton has published grammatical evidence tying Basque to Japanese, or Dene-Caucasic to Eurasiatic if you wish.

Alexandra Aikhenwald proposed something very close to my Borean as part of a larger sub-classification of the world in the 1980s and with pronominal etymologies. As is well known, Edward Sapir stimulated some of these hypotheses with his proposed Sinitic and Na-Dene relationship. I'm sure that I have overlooked many proposals in the 19th century and early 20th.. Prominent among them would be Karl Bouda whose hypotheses came close to uniting everyone from Basque to Gilyak. The work done on finding relatives for Indo-European or the numerous attempts to establish Uralic and Altaic as related to each other and/or Indo-European should be mentioned but the library work would be excessive! However, we should mention Long Ranger Edwin Pulleyblank and his work joining Indo-European and Sino-Tibetan.

Morris Swadesh would call Borean part of his world-wide **trellis** or **network** – La Red Linguistica del Mondo. Part of the 'proof' of this hypothesis would be massive great etymologies, whether of grammemes or basic vocabulary. But another part, one usually not acknowledged by linguists but frequently mentioned nevertheless, would be the cases where individual scholars had proposed parts of the network before. Given the inveterate obsession with binarism among linguists, there may be a large number of proposed two-by-two or threesome genetic connections. These add up to a background support system of small scale etymologies for the overall network or proposed higher level taxon. Basically, the transitivity principle prevails; one cannot easily onceive of Quechua being related to Finnish – this example was once used by Americanist critics of Greenberg – but if they are connected by Mongolian, Gilyak, Eskimo, Apache, Zapotec and Tzotzil, then it is easier to conceive of and accept.

It would be reasonable to suppose that, since all languages ultimately are related to each other, however deep the remoteness between them, that ANY proposed phylum or family or network could be supported by an equal number of small scale proposals. Obviously there are other factors at play: one family may have hundreds of languages, while others have few; one family may have good field conditions and lots of people would work on them; one family could have been known to scholars for a long time while some were practically brand new, e.g., North American languages versus those of central Papua. The small scale etymological support system for Borean is probably the best in the world because these hundreds of languages, as opposed to the thousands involved in the Tropical hypothesis, have been the earliest known, the most studied, and home turf for the majority of linguists.

Addendum: New and Relevant Research in Genetics

However interesting the argument between Klein and Brooks, or Klein and Fleming, there is a definite possibility that both hypotheses or viewpoints may be mistaken. Everything depends on how trustworthy **genetic dating** is or as any experienced archeologist might say – everything depends on the dates. Not just genetic arguments but **everything**. In brief three separate articles appeared in the same issue of SCIENCE this year (May 13, 2005, vol. 308: 965-966, 996, 1034-1036), although a similar genetic study done in 1999 on different chromosomes appeared to disagree with them. Two studies presented original data, valuable data, on Andamanese, Nicobarese, and 'original aborigines' of Malaysia (Semang and Senoi mostly), compared their mitochondrial DNA with those of other peoples in their regions (India and Southeast Asia), with each other and with the basal mtDNA peoples of East Africa. While the Nicobarese quickly disappeared into the rubric of 'just like other peoples of the area', the Andamanese were salient. Exactly what the linguistic genetic classifications show! The 'Orang asli' or aboriginal Malaysians were also distinctive; that is not reflected in their linguistic classifications but it is in **ethnology** which has always treated them as special and probably aboriginal. The third article was by Peter Forster (of the Renfrew Institution) and Shuichi Matsumura; it was an excellent attempt to organize our thinking about the two studies and extend our understanding of the options available in thinking about the great human diaspora. The fourth study was by Mark Feldman of Stanford plus colleagues. It had focused on the Y-chromosome on a global basis and concluded that the 'Adam' of these calculations had probably lived about 40,000 years ago. As Feldman pointed out, however, much of the **dating calculations** depended heavily on the assumptions one made about the **size** of the founding population.

Two things are immediately apparent about the two Indo-Malaysian studies. Their calculated dates for separation/splitting from Africans varied between 60-85 kya. Fundamentally they calculated that the date of the primal haplogroup 'L3' which gave rise to haplogroups 'M' and 'N' and then 'R' was 84,000 years ago, while the dates of the descendant haplogroups were 63,000. Whether these splits happened in Africa before they left or in, say, Arabia on the way or at their arrival near India or when they got to the Andamans or the Malayan peninsula does not get specified. Indeed that would be hard to do, But, indubitably, these dates are just too old to accommodate Klein's wiggly 40 or 50 kya.

It does occur, listening to the American archeologists fussing over early Amerind dates of 11,000 or 12,000 or 13,000, that millennia matter to prehistory. The difference between 40,000 years ago and 50,000 years ago is not trivial. We are not dealing with geological time here. A lot can happen in 10,000 human years! Klein's dates have wandered over a 15,000 year period from 40 kya to 55 kya, when all the time it was kind of obvious that his dating was based on 40,000, the most commonly accepted date for the arrival of the Aurignacians in Europe.. But in order to accommodate the Levantine dates of 45,000 he had to wobble towards 50,000. And so forth. Has Klein's hypothesis been falsified?

The two studies, including Macauley et al cited by Christy Turner on page 20, also argued for analyses of their data which presumed **one migration** from Africa, preferably along the southern coastal route, and then doubling back to Europe. Parts can be specified quite exactly.

First, the Bab el Mandeb must normally be crossed by raft or boat. Years ago I spent a week trying to find a period of low water so folk could walk across the several miles of water. Even during glacial epochs the Bab el Mandeb was just too deep to walk across. However, the gap between Eritrea (really Djibouti) and Yemen is not very large, say 15 miles and one can easily see across the gap. A small raft could probably cross it easily, unless there were strong currents akin to 'spillways' [Überfall, canale di scarico] type that would draw the craft into the Indian Ocean. Most recently, new estimates of glacial effects on oceans have shown that sometimes during the last glacial maximum there **were land bridges** between Africa and Arabia, Eritrea and Yemen.

Secondly, the way to Java is then open. March along the south Arabian coast, indeed going very far inland can lead to trouble like the Rub^c al Khali, 'the Empty Quarter', the largest sand desert in the world. Across the Persian Gulf as far east as Oman, there would sometimes be access to the Iranian coastal strip by land. Thence to the Indus Valley. From India to Sri Lanka and from Burma to the Andamans were walkable at times and from Malaysia to Java too. Then to cross Wallace's Line (of deep water at all times) of necessity by boat or raft, thence to Flores, Timor, other islands leading up to New Guinea, thence walk across the Torres Straits to Australia (sometimes), finally walk to Tasmania (sometimes).

Although Christy Turner seems uncertain on this point, it had to be Greater India where this one-time human migration 'doubled back'. It would almost certainly have meant **either** traversing Afghanistan, leaning westward, up through the Oxus River valley into Uzbekistan and/or Tajikistan, and on to Central Asia. Thence to Europe but also eastward around the Himalayas and on to China, eastern Siberia and the New World **or** doubling back from India into Iran and the Near East across Anatolia to Europe, thence eastwards to America. The second alternative seems to be counter indicated by the archeology of Aurignacian movements given by Bar-Yosef. On the other hand the centrality of India at a date of around 60 kya is supported by the genetic research done by Willems, as reported in SCIENCE, Vol. 306, 17 December 2004, pp.2030-31. Peter Forster is quoted saying Willem's theory "has been gaining a surprising amount of acceptance." [no details are given in the article.—ED] One can only wonder.

I find myself very sceptical of the one trip, **one diaspora by coast**, hypothesis. It all depends on two things. First, that the genetic dates are reliable and second, that their theory of mtDNA connections is valid. I can comment on genetic dates after 19 years of experience with them. They leave a lot to be desired. It requires another geneticist to comment on the analyses in question. I surely am unqualified to do so. So have Fleming's Borean and Tropical hypotheses been falsified ?

A word about the **falsification procedure** in a discipline like ours where four pretty independent fields occasionally confront each other. Klein's Aurignacian theory is mostly archeological but a little bit genetic. Can the dates from a genetic analysis refute the dates from an archeological hypothesis? Fleming's Borean and Tropical hypotheses are mostly linguistic, supplemented by pieces of geography and prehistory and physical anthropology. Can the conclusions from a genetic analysis refute mostly linguistic hypotheses? Everyone can have a shot at answering these questions. Myself, I would answer YES to both questions because that is one good way we can **vigorously test** our hypotheses.

Look forward to the Trombetti article for more thinking along these lines.

The Missing Issue

As mentioned on page 8, no one gave voice to an issue which is apparent in the debate and/or the discussion of it. I find it extraordinarily difficult to put a label on the gist of the issue. Kroeber's **'super-organic'** comes to mind; that was one of the most stirring and convincing arguments against reductionist science or at least against the assumption that human cultural and/or psychosocial things could be reduced to, i.e., explained by, the 'laws' of biology or those of individual psychology. Nowadays the dominant or growing tendency in psychology is to call comparable phenomena **bio-psycho-social** and to stress the interaction or interplay among determinants or factors oriented to biology, psychology and sociology/anthropology. Oddly enough, the study of intelligence, especially human cognitive skills, forms a natural locus for the interaction of genetics (biology), individual experience and attitude (psychology) and group influences (sociology et al). And, when one of the group influences is language, then one brings in linguistics which has its own bio-psycho-social complexities. Intelligence is clearly a matter of genes, as well as access to books and technical know-how, as well as the degree of isolation or cosmopolitan life, and individual disposition. One's status in a social group can have consequences not only for the role demanded of one but also how well one plays the role, e.g., 'you are a new teacher and everyone expects you to know the subject, but maybe not perfectly.'

Normally, archeology concentrates on the narrative it is trying to write about individual cultures and areas and changes through time. It may insert small scale explanations or causal factors from time to time to beef up the narrative. Like 'because humans arrived in Australia circa 45,000 BP they are probably the reason for the major die-off of very large birds in Australia' or 'after the Clovis-wielding humans arrived in North America all kinds of large mammals died off.'

But when Klein asserts that the nearly modern humans (e.g., Qafzeh, Skhul, etc.) probably were not as intelligent as fully modern humans, probably did not have human language, and could not do sophisticated art work either in whittling, sculpting or painting, he has left the narrative and is making major assumptions about human behavior. Apparently one assumption is that evolutionary grade correlates closely with intelligence, with language, and with art. Even though most of the near moderns had virtually the same brain size as moderns and Neanderthals, still their state of evolutionary retardation (my coinage) held them back. Granted that there is a rough and ready correlation between evolutionary grade and cognitive skill – we probably all believe that a little Australopith like Lucy simply was not as smart as Peking Man. We probably could not easily produce good reasons for the belief. But if/when we did, it would very likely include the differences in their tool kits and their brain sizes.

If MHB ('modern human behavior') is bound to the genes, bound to the evolutionary grade, then **why** are there such great differences in technical proficiency, language, and art among members of *Homo sapiens sapiens* or anatomically modern humans now present on earth? And since so much of Klein's case rests on stone-working skills, and that primarily defines his distinction between nearly modern behavior and fully modern behavior, can we not agree that English professors (teachers of English) are inferior to or not as modern as automobile mechanics or airline pilots?

When you read Klein's marvelous book – it will take some time and concentration – you will possibly conclude that he is a born KNAPPER and most likely an expert at it. Imagine us back in 100,000 BC roaming around Galilee. Klein would be the master knapper, Ofer would be an expert hunter or war leader, and I would be a shaman. Three very different kinds of intelligence involved in each vocation. How are we to tell which one was superior to the others? Which was genetically based primarily and which arising more from major cultural interests of our fellow Galileans?

Out of Africa:: Crossing the Bab el Mandeb as Early as 125,000 BP
+ Fossil *Homo sapiens* in Ethiopia circa 195,000 BP

One of the joys of archeology is the excavation of a very strategic site, of a crucial date, done nearly to technical perfection and clearly shedding light on a major problem. That it should be reported in full detail and full awareness of the issues involved is an extra benefit. That the whole exercise should then be commented on, or critiqued as the Americans say, by one of the world's authorities on the pertinent subject is really more than we can expect or possibly deserve!

But that is what we have got in Eritrea on the Buri peninsula near the village of Abdur on the eastern shore of the Gulf of Zula, just a little south of the main seaport of Massawa. The excavating team was led by **Robert C. Walter** (Dep't. of Geology, Centro de investigacion Cientifica de Educacion Superior de Ensenada, Mexico). Other members of the team were **Richard T. Buffler, J.Heinrich Bruggemann, Mireille M.M.Guillaume, Seife M. Berhe, Berhane Negassi, Yoseph Libsekal, Hai Cheng, R. Lawrence Edwards, Rudo von Cosel, Didier Neraudeau, and Mario Gagnon.** The sage commentator on the excavation was **Chris Stringer**.

The report was published in *NATURE*, vol.405, 65-69 (04 May 2000), a surprising five years ago. The reason it was not featured before lies buried in a small maelstrom in one of our heads. The simple announcement part had been remembered but for some reason no one had read the full article or the commentary.

Their Abtract is, as follows:

> The geographical origin of modern humans is the subject of ongoing scientific debate. The 'multiregional evolution' hypothesis argues that modern humans evolved semi-independently in Europe, Asia and Africa between 100,000 and 40,000 years ago.[1], whereas the 'out of Africa' hypothesis contends that modern humans evolved in Africa between 200 and 100 kyr ago, migrating to Eurasia at some later time.[2] Direct palaeontological, archeological and biological evidence is necessary to resolve this debate. Here we report the discovery of early Middle Stone Age artifacts in an emerged reef terrace on the Red Sea coast of Eritrea, which we date to the last interglacial (about 125 kyr ago) using U-Th mass spectrometry techniques on fossil corals The geological setting of these artifacts shows that early humans occupied coastal areas and exploited near-shore marine food resources in East Africa by this time. Together with similar, tentatively dated discoveries from South Africa[3] this is the earliest well-dated evidence for human adaptation to a coastal marine environment, heralding an expansion in the range and complexity of human behavior from one end of Africa to the other. This new, widespread adaptive strategy may, in part, signal the **onset of *modern human behavior***, which supports an African origin for modern humans by 125 kyr ago. [Emphasis added –ED]

1, 2, and 3 are endnote markers for references primarily. They are ignored here.

Rather than write a precis of their admirably concise but densely packed article it will be presented as a series of empirical statements or propositions, all focused on the strategic value of the article.

Where is the site? Right across the Bab el Mandeb or the lower part of the Red Sea from Arabia, specifically Yemen. Along a shore where shell fish may be harvested but coral reef grows. A "soft sandy substrate in a shallow to medium sub-tidal environment" is suggested, say Walter et al.

What was their subsistence economy like? Most ancestral societies are normally presumed to be hunter-gatherer in subsistence economy. But there are large differences among peoples of that type. South African Bushmen and some Californian tribes were probably more gatherer than hunter. The Eskimo on the other hand like some Dorobo of Kenya were mostly hunters. The Abdur people lived off marine shell fish and proper East African game where their blade and flake tools may have been used for cutting up carcasses or possibly as projectile points. The source doesn't give detail on this point. Thus is suggested a subsistence economy more like that of the Eskimo. There is no mention of grinding equipment either. This is probably not a residential site.

What kind of tool kit was found? Large amounts of obsidian flakes and blades along with Acheulian type hand axes. This pretty much characterized the early Middle Stone Age in Africa, the typological equivalent of the Middle Paleolithic in Europe. It is similar to Mousterian which had few hand axes but mostly flakes rather than blades. Typologically, it could be considered ancestral to the 'Mousterian' of Qafzeh, Yet it was not so dissimilar to the somewhat controversial tool kit of the Hobbits of Flores. It could also be considered farther removed but potentially ancestral to the blade heavy Aurignacian tool kit.

What kind of dates go with the site and how solid are they? The dates are very solid, being associated with coral reef dating in several places in the Red Sea from near Sinai all the way to Somalia.. The principal date is **125,000 years** ago with a very modest range of variation over all those Red Sea reef readings. Because of imporant climatic flucuations it is crucial to place the site in the glacial developments of the past 200,000 years. ARL is an inter-glacial site well before (by 40 or 50 kyr) the 'hyper arid' conditions which had such marked effects on Africa, as mentioned by Brooks and Klein during their debate.

What kind of people lived at ARL (Abdur Reef Limestone)? No direct evidence but considerable circumstantial or comparative evidence that these were *Homo sapiens*. There are three lines of argument for the *Homo sapiens* hypothesis. And it is an hypothesis! First, ARL is connected typologically (culturally, not biologically) with South African sites of the same general period where *Homo sapiens* or so-called archaic *Homo sapiens* have been found, above all **Klassies Mouth**. Secondly, most of the East African sites with archaic or controversial Homo sapiens fossils lie in this general time period (150 kyr to 100 kyr), especially **Omo-Kibish** at the other end of Ethiopia, Also to be mentioned here is the pre-cursor, or perhaps ancestor, of *Homo sapiens* found only a few hundred miles away in ^{c}Afar (across the Danakil Depression) in northeastern Ethiopia – *Homo idaltu* of 165 kya. He is sleeping practically underneath ARL. Thirdly, in eastern Africa during this general time period there are **no other kinds** of Hominids or Hominins found by excavators – only *Homo sapiens* or his archaic ancestors. Fourth, it is becoming obvious, and the point was made by Walter et al, that the clear *Homo sapiens* of the Levant (Qafzeh & Skhul) are derived from these African populations of *Homo sapiens*, more explicitly along the Red Sea coast up to Sinai, thence to the temperate Levant, or at least generally northwards via the Valley of the Nile to Palestine and also the north African littoral.

What culture did ARL derive from? That must be one of our ultimate questions because we are getting close to the **seminal** period of the African Middle Stone Age and the most archaic cultural roots of the *Homo sapiens* tribe. Walter et al are inclined to see ARL and related cultures in eastern Africa as innovations and as responses to increasing aridity. Nobody had occupied the shore lines and become marine foragers before. Walter et al were also inclined to cite this as evidence of modern human behavior, allegedly lacking in the early forms of *Homo sapiens*, at least according to Klein. However, what are we going to call an adaptation to lakes and large rivers? Brooks, Yellen and colleagues had reported the presence of harpoons and the hunting of lacustrine mammals, albeit later in time than the general culture horizon of the marine foragers [from South Africa to Sinai –ED]. Ethnographically, we find hippo hunters or fishers of the very large fish (cat fish, etc.) in Ethiopian Rift Valley lakes (Zwai, Margherita, Chamo), as well as Lake Rudolf/Turkana, and possibly earlier in the 'Nil-Kongo Zwischengebiet'. How old these adaptations are or what their relationship to maritime foraging is we do not know. The lacustrine hunters also evolved distinctive types of boats, suggesting that the ARL people may have known how to 'sail' across the Red Sea. In terms of diffusion and contacts the ARL people were about equidistant from both the northernmost of the East African lakes (Lake Tana or Lake Zwai) and the main Nile Valley in the Sudan. [We have ignored the Nile and its developments herein -ED]

What about the glacial periods and the associated climate changes in Africa? Walter et al stress, and rightly so, the fluctuations in climate due to changing glacial periods as key determinants of human behavior in Africa. There are kinds of climate one can adjust to by changing one's behavior (**technology**) and there are kinds of climate one can only adjust to by moving away from it. During the glacial periods there were times when the supply of **water** necessary in basic biological terms for human life **disappears**; then we can say that a certain territory became **uninhabitable**. Walter et al maintain that during the glacial maxima of the last glaciation the lower Red Sea became uninhabitable, almost precisely the time when the only land bridge to Arabia would have occurred. In other words when the whole Red Sea area was practically bone dry then one could walk across to Arabia! But by that time any marine foragers would have left, n'est-ce pas? In fact so dire are the statements about African climate during the period from circa 80 kya to circa 20 kya, made by Brooks and Klein and others, that one has trouble imagining where the surviving human populations actually lived.

Where was there water, when even the great rain forests of Congo and West Africa had shrunk? Well, obviously there were tons of it locked up in Eurasian glaciers and snowy East African mountain tops.Then during the Great Debate Brooks had maintained that there were areas – one might call them oases – were human populations still inhabited the land. Roughly the bulk of East Africa in the narrow British colonial sense – Uganda, Kenya, Tanganyika – plus Rwanda-Burundi and portions of Ethiopia (I presume) got some rainfall and still were sources of Africa's two greatest rivers, Congo and Nile. One indication was that South Africa got re-settled around 20,000 BP from Tanzania; this Brooks and I are wont to correlate with the Khoisan phylum moving south from its homeland in Tanzania. Another, and more definitive, indication is the presence of archeological sites in East Africa during the hyper-arid spell of Africa generally; these were cited by Brooks as part of her argument that cultures were showing more and more signs of sophistication before 40,000 BP. More mundane but more definitive evidence for a wider area is probably found in scholary reports on climate, etc.; we have not consulted them.

Was the climate the principal reason for the great exodus of *Homo sapiens* from Africa? Surely it was not like the exodus of the Hebrews from Egypt to the Levant because that implies that a whole people left one place to settle another. Most likely, it would be more like the Russian conquest of Siberia or the Anglo conquest of North America – a population expanded into new areas, establishing colonies, but keeping the homeland intact – or like the ancient Phoenicians or ancient Greeks colonized various parts of the Mediterranean littoral. The very presence of four major constellations of mankind with distinctive physical-biological and linguistic characteristics not found elsewhere in the world argues powerfully that these represent the people who stayed home, to evolve there each in her own way. So the whole lot of early *Homo sapiens* did not leave Africa – just some did. But the main time periods proposed by various hypotheses from Klein to Macauley to Forster all fall between 80 and 20 kya. One possibility is that borderline populations just oozed over into Asia from time to time, seeking whatever, e.g., new shell fish, more bovid game, freedom from aggressive neighbors, escape from mothers-in-law or patriarchal fathers, etc.While we do not know much about proto-human social organization, it is reasonable to expect – after the work of Alain Matthey and Pierre Bancel – that we had kinship ties and probably the famous universal, the incest taboo.But increasing aridity or variable aridity must have been a powerful motivator for some tribes in some areas. With whole regions going bone dry from time to time the scope of exiting people would have become greater until whole re gions virtually emptied out into neighboring ones. The Sahara, an area the size of the United States, was most prone to dis-habitation and even today lacks a clear linguistic or physical human correlate, although G.P.Murdock tried to show that it was the 'Negro'.

What are the strategic problems or hypotheses which the ARL site affects? [or 'impacts' in contemporary American English –ED] First of all ARL should completely overturn the pooh poohing or denigration of Qafzeh and Skhul which has gained ground with Klein's hypotheses. Whatever MHB 'modern human behavior' meant in the past it means even less now. From the standpoint of psychology and cultural anthropology the concept was hard to define and in fact borderline stupid. (Towards the end of the debate Klein denied that MHB was his phrase or that he had used it.) Secondly, it took 25,000 years for modern man to reach those caves in Israel from Eritrea, but consider where else they would have probably gone during those 25 kyrs.

a) the Red Sea hills which abut the coast get more rainfall than the Nile Valley
b) the Nile Valley via Wadi Hammamat or other streams from the Red Sea hills down to the Nile Valley.
c) The Nile Delta via the north end of the Red Sea and the Gulf of Suez, Cairo is only about 60 miles west and the hills have disappeared.
d) The Rift Valley of the Levant via the Gulf of Aqaba, hence easy access to what is now Israel and/or Jordan. Ergo the Levant and those caves.

e) the west coast of Arabia, usually called the Tihama near Yemen, accessiible by simply following the shell fish back south along the Red Sea shore only this time on the eastern bank. However at Aqaba or here the hunters might have been confronted with some changes in flora or fauna inland.

f) Aden and the Gulf of Aden, turning east towards Oman and India. The Aden area conceivably was a richer shell fish area because of the Indian Ocean's resources but possibly poorer in game animals because of southern Arabia's tendency towards aridity.

g) In any case the road to the rest of Asia is wide open and the outcome does not depend as much on the climate as it did in Africa. The proposed routes of migration proposed by Macauley et al can essentially be confirmed, except that calling this a single migration does not do justice to those who got off the train at Aqaba and Suez.

The date of the arrival at Qafzeh is known archeologically, although the ARL-derived people could easily have been in the Levant earlier than that, say 5000 years. But that is a pure guess. How much time did it take the proposed expansion to reach Aden and turn towards the farther East? In addition let us assume that those working their way along the Tihama could have gone inland and settled western Arabia and then continued east. Just as some of those who went to Qafzeh could have discovered the Tigris-Euphrates river system and followed it towards the Persian Gulf.

If we asume that it took as long to go from, say, Aqaba as it took to go from Eritrea to Qafzeh, then we are being unfair – we have not allowed the time for the initial adaptation to the Levant from Aqaba to Qafzeh. Using the guessed at number from above, we can subtract 5000 years from 25,000 (Eritrea to Qafzeh) and we get 20,000 years to go from Aqaba to Aden. Since our calculated arrival time at Aqaba would be 105 kya, subtracting the 20 kyr from that would have us arriving at Aden **85,000** years ago. Although this is probably an **accident,** given all the assumptions we made, still it agrees strikingly with the 84,000 years estimated for the first split off from the East African basal mtDNA haplogroup 'L3' from the Indo-Malaysian study mentioned on p.31.

Finally, to make our dates a little shakier we have to report that Walter et al noted another layer of tools **underneath** the 125 kyr level. It was not properly dated, nor were its artifacts scrutinized thoroughly or at least they were not reported. This earlier version of ARL could be the one which moved up the coast first. It may have missed out on Qafzeh or Skhul and remained unreported in the Levant. But its southern fellows, we assume, continued on to Aden and arrived there earlier than 85,000 to start the slow expansion towards India.

And postultimately we must add that reaching the Nile Delta leads to reaching the Mediterranean. Whatever they did to the east beyond Palestine is ignored here. To the west the coastal people could have spread their version of the Mousterian along the shores of **North Africa;** this correlates fairly well with the distribution of Mousterian industries cited above from Klein. Theoretically, the ARL people could be the source of the modern fossil found at **Jebel Irhoud** in Morocco

However, there is no reason in principle that we know of that would have prevented events from taking a course **opposite** to the one outlined above. The coastal people could have started on the Mediterranean and worked their way south along the Red Sea. A few more fortunate sites like the one at Abdur could **settle** this question.

Chris Stringer's Commentary from *NATURE* 405, 24-27 (04 May 2000) was entitled **"Paleoanthropology: Coasting out of Africa"**. Excerpts from it here below:

> It is now generally accepted that Africa is the ancestral homeland of modern humans, *Homo sapiens*[1-3]. But the timing of human dispersal from Africa, and the routes taken, remain controversial. Was there only one major dispersal, which took place after 50,000 years ago[1]? Or were there several episodes of migration, starting earlier and covering a longer period of time[2]? Moreover, was the most obvious route taken – through Sinai and the Levant (the eastern Mediterranean coast) – or might there have been other pathways?
> ...Walter et al[4] present the first well-dated evidence that humans were living along the African coast of the Red Sea during the last interglacial, around 125,000 years ago, and were probably exploiting marine food resources. They argue this finding implies that there was a major change in human adaptive capacities about that time. It may also indicate that coastal routes were used by early *Homo sapiens* to leave the African homeland. Other shorelines of Late Pleistocene age (oxygen isotope stages 2-5 ....) might therefore preserve further traces of our exodus from the continent.
>
> [Figure 1 "Chronology of the late Pleistocene" – is not included here –ED]
>
> At one time it was believed that humans began to exploit marine resources only during the Eurasian Upper Paleolithic, after 40,000 years ago[5]. But it seems that both early modern humans in southern Africa and Neanderthals in the Mediterranean did this during the Middle Paleolithic[1,6], at sites such as Klassies and Herold's Bay Caves in South Africa, and Vanguard Cave (Gibraltar) and Moscerini Cave (Italy), respectively ... However, much of the Late Pleistocene evidence of these seaside dwellers must now be submerged beneath the high sea levels that have pertained during the present interglacial which started some 12,000 years ago. Because of their elevated altitude, due to the higher sea levels of that time, sites of last interglacial (early oxygen stage 5) age are more likely to have remained exposed. This is particularly true where the land has been uplifted by geological processes, as is the case around parts of the Red Sea.
>
> [Figure 2 "The Old World as it might have looked during the Late Pleistocene, round 65,000 years ago" –is not included –ED]
>
> Walter et al[4] recovered Middle Stone Age (African Middle Paleolithic) artefacts, such as hand axes and obsidian flakes, from strata in a raised fossil reef near Abdur in Eritrea. Geomorphological considerations and correlation with other Red Sea locatities suggest that the site dates to the last interglacial. Walter and colleagues confirmed this age with uranium-series dates that, on average, gave a figure of about 125,000 years. Who made the artefacts is unknown. But there are fossils of near-modern or modern *H.sapiens* from around this time[1,2] in neighboring regions such as Ethiopia (Omo Kibish), Sudan (Singa), Kenya

(Guomde) and Israel (Skhul and Qafzeh). So it is likely that the people concerned were early members of our species.

Klein[1] has argued that the main despersal of modern humans from Africa probably occurred only after the beginning of the Later Stone Age (equivalent to the Eurasian Upper Paleolithic). For him, that event heralds the beginning of modern cognitive and adaptive capabilities. Acording to this view, then, the presence of modern humans in the Levant during the last interglacial, represented by the burials at Skhul and Qafzeh, was only a brief geographical extension of the species from Africa. The real dispersal of *Homo sapiens* was through that region, but did not occur until the Upper Paleolithic, perhaps 45,000 years ago.

Other workers have favoured an earlier, Middle Paleolithic, beginning for dispersals. Kingdon[7] proposed that Middle Palaeolithic people left Africa through the Levant and reached southeast Asia by 90,000 years ago. There they adapted to coastal conditions, and developed a boat- or raft-building ability that enabled them both to return to Africa and to move southwards to Australia. By contrast, Lahr and Foley[2] suggest in their 'multiple dispersals model' that a more direct route from Africa to Arabia and further east could have been taken before 50,000 years ago, perhaps using the coast. However, subsequent dispersals to the north, evidence for which comes from early Upper Palaeolithic artefacts found in countries such as Egypt, Israel and Bulgaria, would have followed the Levantine route

The findings of Walter et al[4], together with new data from Australia, allow further elaboration of these possibilities. There is increasing archaeological evidence[8] that Australia was colonized (by boat, because no landbridges existed during the Pleistocene) before 50,000 years ago – that is, before the proliferation of Later Stone Age and Upper Paleolithic features such as blade tools and art. Moreover, a modern-human burial site from southeastern Australia, associated with the symbolic use of red ochre, as been re-dated to about 60,000 years ago[9]. This implies that at least one dispersal of modern humans from Africa must have occurred during the Middle Palaeolithic, and that characteristic elements of modern-human behaviour existed by then.

We can now add Walter and colleagues' discoveries[4] to this picture. Middle Palaeolithic people might have spread from Africa along the shorelines of Arabia and into southern Asia during, or soon after, the last interglacial. Continuing along the narrow shorelines, to which they were already adapted, they could have progressed all the way to Indonesia at times of low sea level... Movement along the coasts meant that they could have been spared the degree of habitat disruption faced by inland population during the rapid climatic fluctuations of the Late Pleistocene. Coastal migration might also explain why they did not immediately replace archaic peoples living inland, such as those known from Ngandong in Indonesia[1]. At what stage the coastal migrants first ventured out to sea is unknown, but from the Australian evidence it seems that it must have been before 60,000 years ago. Such behaviour may have developed through the need to ford rivers or extend coastal foraging areas.

Archaeologists might now concentrate profitably on exposed fossil beaches in regions such as Arabia and India. Such sites may well contain Middle Palaeolithic artefacts that further document the spread of modern humans and their adaptations to a coastal environment. Southern Asia must have formed an important secondary centre for dispersals of modern humans – there, too, it may have been the coasts that provided the first and fastest routes for migration, before movement inland up river valleys.

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End of Stringer Excerpts

Three things which were not known to most of us, or not discussed much, were the fossils at Singa and Ngandong and the finalizing of the dates of red ochre painting from Australia. (1) according to a map in the source mentioned in Stringer, namely Klein, **Singa** is in the eastern hill country of the Sudan up next to Ethiopia and close to the Blue Nile; that country is the old home of the Fung, the several peoples called Pre-Nilotes by Grottanelli and Murdock who, reflecting Carleton Coon's ideas, thought the Singa fossil was 'Bushmanoid' , (2) the **Ngandong** fossil is from the Solo river area of central Java, home to many famous often controversial discoveries in the history of physical anthropology, e.g., 'Java man', 'Pithecanthropus erectus', etc. The Ngandong fossil which Stringer refers to as 'archaic' human is called a 'late Homo erectus' by Klein. It is circa 250,000 BP. (3) the dating of the **red ochre** site from southeastern Australia has been up in the air for so long that most of us forgot about it; the meaning of red ochre painting (or dusting?) is hard to grasp. It must mean something, probably symbolic, but what? The 60,000 ya date is **decisive.** Southeastern Australia is a long way from Wallace's Line.

A SHORT ANNOUNCEMENT

In a letter to NATURE 393, 458-460 (04 June 1998) Ernesto Abbate and sixteen colleagues, primarily from Italy and Eritrea, announced in their Abstract:

"A one-million-year-old Homo cranium from the Danakil (Afar) Depression of Eritrea."

> One of the most contentious topics in the study of human evolution is that of the time, place and mode of origin of Homo sapiens. The discovery in the Northern Danakil (Afar) Depression, Eritrea, of a well-preserved Homo cranium with a

> mixture of characters typical of *H.erectus* and *H.sapiens* contributes significantly to this debate. The cranium was found in a succession of fluvio-deltaic and lacustrine deposits and is associated with a rich mammalian fauna of early to early-middle Pleistocene age. A magnetostratigraphic survey indicates two reversed and two normal magnetozones. The layer in which the cranium was found is near the top of the lower normal magnetozone, which is identified as the Jaramillo subchron. Consequently, the human remains can be dated at 1 million years before present.

Clearly we need more information about this, and what appears to be an unusual dating system. It is there on three pages in *NATURE,* but it is too far from our current set of problems. But see the next entry, below.

Fossil Homo sapiens in Ethiopia circa 195,000 BP

In the hot lowlands where the cool Ethiopian highlands drop off and their waters flow down into the Rift Valley and its large lake, Rudolf or Turkana, the principal river is called the **Omo;** its tributary system has drained much of the entire block of the southwestern highlands. Where the Omo debouches into Lake Rudolf is basically the modern cultural border between Omotic and Cushitic peoples on the one side and East Sudanic branches of Nilo-Saharan on the other. Here at this strategic confluence was found one or two of the most important fossils in Africa, **Omo I and Omo II** as they were called.

Although Omo I and Omo II were excavated back in the 1960s and a tentative date of 130,000 years assigned to them, their true or proper date has long been absent, even if wished for. The two were some of the first *Homo sapiens* fossils, or archaic *Homo sapiens* fossils, found in Africa and had a serious effect on prehistory, constituting one of the reasons for the **Out of Africa** hypothesis from the beginning. According to Richard Klein in his massive comparative study of African Homo fossils – in *The Human Career,* Chapter 5 – in Omo-Kibish the human fossils called 'Omo I' were nearly modern humans, while another related fossil, Omo II, was more archaic. They were essentially contemporaries, i.e., not more than 5000 years apart, but have remained anomalous. Or to put the matter in Linnean terms, Omo I would probably be classified as *Homo sapiens sapiens,* while Omo II would be *Homo sapiens avunculus,* an older but related version of *H.sap.sap*. However, despite their stylistic differences, Omo I and Omo II had the same cranial capacities or brain sizes (1400 cc, fully in the modern range but also matched by Neanderthals). And while Omo I is not the earliest of the Homo sapiens tribe (the archaic or early modern member fossils), Klein places her as one of the earliest of the nearly modern group. The honor of being one of the earliest of the archaic tribe goes to *Bodo,* another Ethiopian but over the mountains and down towards Afar in the so-called Middle Awash (river) basin. Bodo is around 600,000 years ago.

Now, however, an answer has come to the question of the proper ages of both Omo I and Omo II. Ian McDougall of the Australian National University in Canberra and colleagues John Fleagle of Stony Brook University (NY) and Francis Brown of the University of Utah announce in a recent issue of *NATURE (*vol.433, 17 February 2005,733-736), also summarized in the Boston Globe of the same date page A3, that a more definitive date of 195,000 BP had been obtained. They re-visited the sites of Omo I and Omo II, "analyzing the geology and testing rock samples with more modern dating techniques" To find the age of the skulls they "determined that volcanic rock lying just below the sediment that contained the fossils was about 196,000 years old. They then found evidence that the fossil-bearing sediment was deposited soon after that time."

Some experts rounded up by the newspaper commented as follows: **Paul Renne** of the Berkeley Geochronology Center, which specializes in dating rocks, said the researchers made "a reasonably good argument (to support the dating of the fossils); it's more likely than not" Their work was "very exciting and important." **Rick Potts** of the Smithsonian's Human Origins Program said their case was "very strong. They're right on the cusp of where the genetic evidence says the origin of modern humans ...should be." G. Philip Rightmire of S.U.N.Y., Binghamton (NY) said he believes that "the Omo fossils are of *Homo sapiens* and a more primitive ancestor. The find appears to represent the aftermath of the emergence of *Homo sapiens*, when it was still living alongside its ancestral species." [Does this remind you of *Our Primitive Contemporaries* ? –ED]

The most interesting part of this is contained in Rick Potts remarks about the genetic evidence. Since we began almost 20 years ago, the genetic analyses have been producing these ostensibly imaginary but surely too ancient dates for the birth dates for either Adam or Eve. These mythical figures, of course, stand for the hypothetical start of the branching off or descent process in the world wide human populations genome. A linguist would call it proto-man or proto-woman, the time when our species began to diversify or at least the genes belonging to proto-man and proto-woman began to reproduce themselves, have mutations, and to diversify.

But, I confess, I took those hypothetical beginning dates for *Homo sapiens sapiens* as being somewhere between a wild guess and a phantasm! I have been corrected! **The possibilities of establishing good correlations between solid paleo-anthropological or archeological dates and calculated genetic dates is exciting and could help us a great deal.** Let us hope it all works out! And let us hope that one day the long beleaguered and controversial **linguistic chronology** can join these happy correlations!

The matter of these re-analyzed Omo sites being highly technical and done by geologists, it is useful to reproduce their Abstract, just to be safe!

Stratigraphic placement and age of modern humans from Kibish, Ethiopia

(Authors'names and addresses omitted)

In 1967 the Kibish Formation in southern Ethiopia yielded hominid cranial remains identified as early anatomically modern humans, assigned to Homo sapiens[1-4]. However, the provenance and age of the fossils have been much debated[5,6]. Here we confirm that the Omo I and Omo II hominid fossils are from similar stratigraphic levels in Member I of the Kibish Formation, despite the view that Omo I is more modern in appearance than Omo II[1-3]. ^{40}Ar/^{39}Ar ages on feldspar crystals from pumice clasts within a tuff in Member I below the hominid levels place an older limit of 198 ± 14 kyr (weighted mean age 196 ± 2 kyr) on the hominids. A younger age limit of 104 ± 7 kyr is provided by feldspar from pumice clasts in a Member III tuff. Geological evidence indicates rapid deposition of each member of the Kibish Formation. Isotopic ages on the Kibish Formation correspond to ages of Mediterranean sapropels, which reflect

increased flow of the Nile River, and necessarily increased flow of the Omo River. Thus the $^{40}Ar/^{39}Ar$ age measurements, together with the sapropel correlations, indicate that the hominid fossils have an age close to the older limit. Our preferred estimate of the Age of the Kibish hominids is 195 ± 5 kyr, making them the earliest well-dated anatomically modern humans yet described.

['sapropel' is 'a fluid slime found in swamps as a product of putrefaction' or 'a mud rich in organic matter formed at the bottom of a body of water' From Greek *sapros* 'rotten' + *pelos* 'mud' –ED]

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Quotation of the month, kindly supplied by John D. Bengtson

"Science the process is objective, but scientists are people, and they aren't objective."

Jacques Gauthier, Paleontologist

A Report On A Possible Pre-Clovis Site In Georgia (USA)

There was a great but brief commotion on American television earlier this year in honor of a great archeological discovery near Savannah, Georgia. A kind of great electronic SHOUT something like 'Eureka!' was heard for just a moment and then it subsided. I could not even find notice of it the very next day in the daily newspaper. When I called knowledgeable people to see if any knew anything more about the great discovery, I found that nobody had heard a thing about it! "This is remarkable", said I to self, "they said on television that Amerind tools and bones not less than 50,000 years old had been unearthed near Savannah, Georgia."

Thereupon I phoned my good friend and colleague, Dr. Larry Lepionka, of Charleston, South Carolina, to see what he knew about this. If Larry had not heard about the great discovery, then I reckoned that it was a lost cause. Larry, it turned out, knew about the site and knew the excavator quite well and could get some particulars about the whole business fairly quickly. And thus Larry found out that the great discovery was somewhat less than that. He has reported what he found out – to us (overleaf)

Larry studied anthropology at Boston University, among other places, and got his doctorate in archeology (but still in anthropology) at Harvard, doing field work among the Khoisan of south Africa. Larry's style is careful and precise and he does not rush to conclusions. Those qualities hold him in good stead amongst the modern Zeitgeist of Americanist archeology.

We thank him for his contribution.

THE PRE-CLOVIS HORIZON: THE TOPPER SITE

Larry Lepionka
College of Charleston

Recent archaeological work at the Topper Site, Allendale County, South Carolina, has received considerable publicity, based on the definition of its basal deposits as pre-dating the well-established Clovis period, i.e. earlier than 11,500 radiocarbon years before the present. Claims of up to 50,000 years have been made in the popular press, and it is critical that we note that the on-site investigators make no such claim. Their excavation and analysis is, in fact, far more solid and substantive than much of the resulting publicity, and will be reviewed below.

In brief summary, it is generally agreed that the Clovis lithic industry is the earliest broadly recognized evidence of human occupation of the New World. The elegantly made fluted points that are the instantly recognizable defining trait of the industry are found all across North America, appearing within a relatively short span of terminal Pleistocene time. Some elements of the lithic industry may have correlates in Old World assemblages (e.g., the Siberian Dyuktai), but the fluted point is an original, to all appearances a technological innovation made in this continent, an argument strengthened by generally late appearance of Clovis forms in Alaska and the northwest. Together with related industries (e.g., Folsom), Clovis constitutes the Paleo-Indian period in the North American archaeological sequence.

Paleo-Indian artifacts have been found in the presence of extinct megafauna–mammoth, mastodon, giant bison–and were the first materials found that unequivocally proved human occupation of the New World during terminal Pleistocene times. Dates for these sites, however, do not exceed 11,500 radiocarbon years. The question of what–if anything–preceded Clovis has been a tantalizing issue for decades. Scholars, with that lack of objectivity that can be all too typical of us, generally have fallen strongly on one side of the question or the other–absolutely nothing came before, or there were tens of thousands of years of human occupation.

Those against pre-Clovis occupation do have a powerful argument–if not a total absence, at least a great paucity of evidence for such occupation. Many claims for such sites have been made, and the vast majority have been discredited–either the dating is fallacious or the deposits in question are acultural or derived. Nevertheless, a few sites have maintained their credibility–in fact have been made more credible by the strong attention and resulting work focused on them. Among these are the enigmatic Monte Verde in Chile and the Meadowcroft rock shelter in Pennsylvania. To these are now added the Topper site.

Al Goodyear, Director of the Allendale Paleoindian Expedition and principal investigator of the site, has been kind enough to make available to me an advance copy of his contribution to *Paleoamerican Origins* (Bonnichsen, Lepper, Steele, Stanford, Warren, Gruhn, Editors, ISBN 1-58544-366-2, Texas A & M University Press, May 2005). In this, he reviews findings at Meadowcroft and at Cactus Hill and Saltville (both in Virginia), and gives his own analysis of the Topper site. All of these sites provide credible evidence for a human presence prior to 11,500 radiocarbon years ago.

The pre-Clovis component at Meadowcroft is distinguished stylistically, stratigraphically, and by radiocarbon date from the overlying Clovis deposit, though its lanceolate points bear considerable similarity to Clovis forms. Overall assessment of the dates indicates a range of 12,000 to 15,000 radiocarbon years. The Saltville site has comparable dates, ranging from 13,000 to 14,500 radiocarbon years and contains a midden feature and other floral and faunal remains, including worked bone. Cactus Hill dates range from 15,000 to 17,000 radiocarbon years (dates replicated by optically stimulated luminescence analysis (OSL)), and are derived from hearths containing lithic debitage that are situated 10 to 15 centimeters below the Clovis level.

The Topper site (38AL23) is located on a Pleistocene terrace of the Savannah River in a region long known for its chert quarries, which were extensively exploited throughout the prehistoric period. It contains an upper component (1 to 1.4 meters depth) of colluvial (slopewash) soils containing cultural materials, in appropriate sequence, from Clovis to the 18th century. Beneath it, to a depth of ca. 2.2 meters are alluvial (river deposited) sands lying unconformably on a 2 meter thick river-scoured silt-clay deposit of Pleistocene origin. The pre-Clovis artifacts are found in the sand immediately above the clay stratum, well separated from (i.e., below) the Clovis layer. They occur in discrete clusters with no evidence of bioturbation or other disturbance. The contact level between the two strata is dated by OSL to 15,200 ± 1500 years before the present.

A further separation occurs upslope, where a distinct and sterile colluvial deposition is situated between the pre-Clovis sand and the Clovis-bearing colluvium. It is estimated that this upslope deposit took 2000 to 4000 years for formation. Using these estimates and the mean value of alluvium/colluvium contact date, the pre-Clovis alluvium was in place by 17,200 to 19,200 years ago, with its basal elements (containing the artifacts) necessarily somewhat earlier. Goodyear suggests a range from 16,000 to 20,000 years before the present. The underlying silt-clay terrace yields only infinite C-14 dates (over 50,000 years), so offers no useful further time delimitation.

The stratigraphy and the internally consistent OSL dates mark a clear break between pre-Clovis and Clovis, and provide a credible time range for the former. (One curious factor: The upslope colluvium whose formation time is utilized for estimation of the age of the pre-Clovis alluvium has, in the published soil profile, no artifacts directly beneath it. If there are indeed none there, this is an issue the archaeologists might ponder, for it means that "pre-Clovis" is found only directly beneath Clovis, and not elsewhere on the available surface of that time.)

What, then, of the artifacts? Floral and faunal remains are not preserved in the alluvial sand (hence the absence of radiocarbon dates for this stratum). Artifacts (totals not provided, but said to be in "the hundreds") consist of clusters of broken chert where cobbles were processed. Neither bifaces nor debris from biface production are present, and much of the assemblage is "microlithic". While much of this material is no doubt true debitage, the waste products of core smashing, there are clearly recognizable artifact forms and indications of retouching.

Evidence for actual use is limited, with only 6 out of 50 better preserved pieces yielding microscopic evidence of possible utilization. However, it must be noted that chert is very subject to weathering, its clear glass-like surface becoming pitted into a friable coating that obscures markings of both use and manufacture. This is the case with many of the recovered specimens.

Discussion:

What then, do we have here? The concentrations of chert, the position of chert cobbles away from their immediate locus of provenience, and the evident working of many of these pieces provides evidence of human presence and activity at a time prior to the Clovis occupation. The quality of excavation and analysis by Goodyear and his associates confirms this central issue but, as always, generates far more questions to answer.

For example, Goodyear notes that the presence of bifaces at Meadowcroft and Cactus Hill and of biface debitage at Salt Hill suggests possible development into a Clovis industry, but the Topper artifacts with their strong microlithic component do not readily fit into such an evolutionary scenario. Have we then a separate industry, with its own distinct cultural history? Impossible to say, until further such sites are located, or further work at Topper itself provides more data.

As presently known, the site is restricted to lithic evidence–in the absence of faunal (including *Homo sapiens*) and floral remains, of structural elements such as hearths, or utilization evidence for the stone itself, the site is thus far severely restricted in what it can tell us about the past. Nevertheless, its date is alone significant, as well as the direction that it can give future research.

On a more abstract level, this site and the others reviewed by Goodyear can be looked at from the perspective of what seems to be "reasonable". We have noted the divide between those who firmly advocate "nothing before Clovis" and those who see tens of thousands of years of antiquity for Man in America. Fifty thousand years is an oft quoted figure–and this does truly strain credibility–the problems with it include (but are not limited to): 1) Old World dates for the dispersal of modern humans–100,000 years ago for the first evidence of movement (barely) out of Africa, and 40,000 years for ubiquitous presence in the Old World; 2) the absence of cultural evidence before 25,000 years ago for occupation of eastern Siberia, and the implausibility of an earlier maritime technology that would allow the crossing of oceans; and 3) the extreme paucity (at least thus far) of pre-Clovis sites, whereas evidence for much more ancient and primitive cultures and forms of humanity are abundant and long established in the Old World.

On the other hand, there is the flaw of the literal "Clovis-first" argument. Obviously, the Clovis people came from somewhere. And, given the techno-ecological limitations of a foraging economy, in small groups–perhaps only a few such, or perhaps in a continual stream over a long period. It would have taken time, but given unchallenged access to the heretofore unexploited and immensely rich ecology of America south of the glacier, they could have soon burgeoned into the broad distribution that we perceive in their artifact scatter across the continent. But the farther we go back toward these origins–the initial immigrations–the fewer the people would be, and the fewer their sites would be.

It should not be surprising that we see a kind of "Cambrian explosion" as these populations take hold and expand–and, to all appearances, invent the fluted point, with its lanceolate form that is foreshadowed in some of the "pre-Clovis" sites.

And how much time is reasonable? Note that none of the sites discussed by Goodyear claim any age in excess of 20,000 years–for most, in fact, it is considerably less. Even the most ardent "Clovis First" advocate must acknowledge some period, albeit possibly very brief, before the continent-wide dispersal of fluted points is possible. That is very likely what we are seeing in these "pre-Clovis" sites. They certainly are not sufficient at this time to argue for a wide diversity of cultures and origins in the peopling of the Americas. We must acknowledge, however, that what is "reasonable" can and often has been turned upside down by actual discovery.

It is unnecessary to say much about the issues involved in the prehistory of the New World, at least not after Larry Lepionka's excellent and succinct discussion.
There are just three additional observations which can be made to supplement Larry's paper empirically. I would mention the following:

1) There eventually was a newspaper report on the Topper site, made by John Noble Wilford of the New York Times but the report was published in the International Herald Tribune (July 1, 2004, page 10) and entitled "Prehistoric blades as cutting-edge find". Since Wilford is or was the New York Times' star reporter for science news, the implication is that someone in New York took the discovery of the Topper site seriously and sent their star reporter down to South Carolina, where the excavator lives, to get a full report. While Wilford's report gives much detailed information about the site, it adds little to what Larry has told us – except for two things. First, that Robson Bonnichsen, a long ranger formerly of the University of Maine, then Oregon State, and now Texas A&M, has visited the site and given his assessment. (Over the years our **newsletter** has reported on Bonnichsen's analyses of many American sites.) Second, Wilford quotes Albert Goodyear as saying: "If this is **25,000** years old, and **I think it is**, then scientists will come here from all over the world to see for themselves. And they will argue about it for another 10 years."
2) An archeological site from far eastern Siberia way up in the really cold region not so far from the Bering Straits has been reported in SCIENCE (2004). As relayed by the Boston Globe (Jan. 2, 2004, p.A2) Russian archeologists found a site in the Yana river area with "stone tools, ivory weapons, and butchered bones of mammoths, bison, bears, lions, and hares, all animals that would have been available during that Ice Age period." The Yana flows between the Cherskiy and Verkhoyansk ranges of mountains and into the Laptev Sea; it is around 1000 miles from the Chukchee peninsula. That is a shorter distance than Polar Eskimos walk during their summer time migrations (John 'Jack' Campbell, archeologist, personal communication, he walked the walk) Apparently radio carbon dated to **30,000 BP** this is a Pleistocene site during high glacial times. Yet the Yana area was a **frozen tundra but not glaciated,** even though its region normally has the northern hemisphere's coldest temperatures in the winter. An adaptation very similar to the Lapps, Samoyeds or Chukchee comes to mind. The Yana area seems to be another 'extreme environment' but without agriculture or pastoralism. So that the Eskimos come to mind after all. The Yana site also reinforces the theory that the original Amerinds were landlubbers instead of coastal folk and that they were big game hunters –like the Eskimos. I cannot help thinking that they were probably speaking a daughter language of **early Borean.**
3) One cannot help wishing that **Scotty MacNeish** had lived to hear about the Yana river site, the Topper site, two sites in Virginia and the acceptance of Monte Verde in Chile. Scotty's site in New Mexico had dates in the 20,000s and impressive finds like an animal bone with an arrow head embedded in it. Yet the **self-appointed** referees and judges in American archeology dismissed his site a while back and I have not heard about it since from an archeologist. The referees tried to dismiss **Anna Roosevelt's** site in Amazonia because it was 'not up to standard', until she replied that they had 'raised the bar' so high that their own Clovis sites could not qualify!

ARCHEOLOGY IN THE GREAT RAIN FORESTS?
A Surprising Affirmative

Can you list or mention, if asked, **two or more** archeological sites which were/are located in one of the world's three great rain forests – the central African, the southeast Asian, or the Amazonian? I am reluctant to admit that I can only think of **one** (Anna Roosevelt's Amazonian site which was written up in our Newsletter). We should probably also stipulate **tropical rain forest** because there are some great forests which are not tropical or not rain forest, e.g., southwest Ethiopia's 'montane' or highland forest, the vast taiga of Russia /Siberia, or the old Northwest Coast of Canada/USA. Also those areas of northern Australia and highland New Guinea which are teeming with diverse cultures and languages are also heavily forested, as was Madagascar until recently. But like the Amazon the forested areas of diverse humanity in New Guinea also feature **forest-adapted agriculture.** As always what we are really looking for is older, pre-agricultural, pursuant to our basic research goals.

When a topic is fairly new or unusual, it is useful to surround it with some background. In this case the whole topic is covered in a remarkable book, edited by Alison Brooks' colleague at George Washington University, **Julio Mercader**. Its title is *Under the Canopy: The Archaeology of Tropical Rain Forests.* 2003. Rutgers University Press, New Brunswick, New Jersey and London.

The background is admirably presented in Julio's introduction, in two parts. The first part is drawn from Colin Turnbull's outstanding book *The Forest People: A Study of the Pygmies of the Congo* which was very popular in undergraduate classes in Anthropology for a long time. You have to imagine a 6'3" blue-eyed Scottish laird somehow squirming through the jungle trying to keep up with his Pygmy friends, a feat which Colin proudly announced that he learned to do adequately. (If you've not read *The Forest People,* you ought to –ED) Perhaps because he was a cultural anthropologist, rather than an 'objective' type from the physical disciplines, Colin made friends with the wee folk and deeply absorbed much of their world view. My friend Colin is dead now but he left this powerful contribution for us to reflect on, as quoted by Julio Mercader on page 1.

> "Almost exactly in the middle [of the tropics] ...lies...a vast expanse of dense, damp and inhospitable-looking darkness ...Anyone who has stood in the silent emptiness of a tropical rain forest must know how ...[people] coming ...from an open country ...of sunlight ...[must feel]. Many people who have lived there, feel just the same, overpowered by the heaviness of everything –the damp air, the gigantic water-laden trees that are constantly dripping, never drying out between the violent storms that come with monotonous regularity, the very earth itself heavy and cloying after the slightest shower. And, above all, such people feel overpowered by the seeming silence and the age-old remoteness and loneliness of it all."
>
> "But these are feelings of outsiders, of those who do not belong to the forest. If you are of the forest it is a very different place. What seems to other people to be eternal and depressing gloom becomes a cool, restful shady world with light filtering lazily through the tree tops that meet high overhead and shut out the

> direct sunlight –the sunlight that dries up the non-forest world of the outsiders and makes it hot and dusty and dirty."
>
> "Even the silence is a myth. If you have ears for them, the forest is full of sounds –exciting, mysterious, mournful, joyful ...And the most joyful sound of all ...is the sound of the voices of the forest people as they sing a lusty chorus of praise to this wonderful world of theirs –a world that gives them everything they want ...But if you are an outsider from the non-forest world ...this glorious song would just be another noise to grate on your nerves."

Four things are relevant to this picture of the forest and the Pygmies. (a) Pygmy music is a basic African **polyphony**, supplemented by pipes and flutes cut from the nearest (appropriate) branch, (b) Pygmy religion lacks the sky-god and earth-mother so common in African religions; the Pygmies venerate the **forest** itself or to put it less accurately, their deity is the forest; (c) ColinTurnbull wore proudly a **blue mark** on his forehead, engraved like a tatoo; it was the sign or symbol of a Pygmy initiation rite, (d) the Mbuti Pygmies of the Ituri forest were fairly light in skin color, by my observation and those of others; in this respect they were more akin to the Bushmen and many Ethiopians; thus three of the four primary genetic clusters proposed for Africa by modern geneticists (African Negro, Pygmy, Bushmen, Ethiopian) were not so different from the bulk of non-African mankind in this respect.

Julio Mercader in the second part of his initial presentation waxed eloquent on the topic under discussion here. His remarks are quoted here (from pages 1-2):

> "As Colin Turnbull noted in 1962, outsiders tend to depict rain forests as impenetrable worlds of chaos, timeless 'jungles' in which ancestral plants, creatures, and humans are trapped. **Romantic clichés** portray counterfeit descriptions of a pristine jungle, unchanged through time, in which animals and plants are described with a plethora of aggrandizing superlatives. Contrarily, the human beings that inhabit this frozen homeland of botanical and zoological wonders are referred to with degrading epithets allusive to atavistic cultural features inherited from a timeless prehistoric past.
>
> It is no surprise that archaeologists have shown little interest in discovering the prehistory behind the trees. Popular re-creations of early 'primitive' life in the jungle draw on **myths** from nineteenth-century travel accounts, old ethnographic reports, and novels and perceive the forest as a barrier to 'civilization'. Thus, the early settlement of rain forests would be of little interest. Secondly, the tropical forest is often viewed as an **extreme** environment whose settlement requires great cognitive skills and technological endowment; that is, a high-risk and unhealthy ecosystem that was avoided by early hominids (Bar-Yosef and Belfer-Cohen, 2000), first colonized by anatomically modern humans (McBrearty and Brooks, 2000) or farmers (Bailey et al, 1989). Thirdly, there is the stereotype that the prehistory of the tropical forest is **unknowable** through archaeological research, given that organic remains decompose quickly in these environments, and potential archaeological materials would disintegrate and vanish in the **acidic** rain forest soils...Yet, archaeological data to support or

> refute the above-described popular and academic assumptions have been totally lacking til recently. [Emphases added –ED]
>
> *Under the Canopy* indicates that prehistoric foragers were fully capable of a long-term occupation of tropical forests and that, by **late glacial times**, the settlement of the world's rain forests was already well established.Uninterrupted human occupations for centuries must have inflicted a human signature on the makeup, structure and geographical distribution of rain forests. Far from being pristine jungles, tropical forests today may be variable products of human and natural forces. And tropical forests in the past may be far from having modern analogues, as they occurred under Pleistocene climatic regimes not prevalent today. This is an important aspect to be observed when researching the many ways in which prehistoric groups responded to prehistoric ecosystems. The continuous and repeated inhabitation of the rain forest by prehistoric hunter-gatherers over hundreds of generations have brought about a tight interaction between human and biotic communities (Head, 1989; Piperno, 1994, Bush and Colinvaux, 1994), sometimes influencing tropical forest species composition through the use of fire ……. [There are seven references after 'fire' –ED]

In a section entitled **"Hunting and Gathering in Tropical Rain Forests: *Was* It Possible?"** Mercader goes to the heart of the issues, swimming apparently against a growing and contrary consensus, as follows

> Perceived environmental and diet constraints in today's closed forests, especially **lack of wild carbohydates** (Bailey et al, 1989), have sustained current anthropological depictions of the early prehistoric settlement of the tropical forest. It has been demonstrated that present forest dwellers do **not** live independently of farming. Therefore, the ability of prehistoric foragers to subsist in tropical forests on purely hunting and gathering grounds was questioned …. [seven references from 1986-1997 –ED] As a result, anthropological models portrayed dense tropical forests of the Holocene as unfriendly environments **unable** to support prehistoric foragers before the advent of **farming ….**[four references –ED**]**. Prehistoric hunter-gatherers, thus lived in tropical forests for the last few millennia, only after farmers colonized rain forests and enhanced a naturally low productivity by farming and subsequent environmental alteration of closed-canopy forests (Bailey et al, 1989:73) The farming modification of the forest brought about a wider availability of game, which, in turn, made hunting and gathering feasible. This theory, referred to as the **'null hypothesis'**, has very important implications for human evolution. These implications are (1) early humans lacked the capacity to settle extreme environments; (2) extensive population deserts existed throughout the wet tropical belt during the entire Pleistocene and most of the Holocene; (3) inherent human inability to colonize and live on rain forests was overcome during the late Holocene; (4) global colonization by archaic humans was highly differential and excluded tropical forests; and (5) hunter-gatherers were incapable of indirect or direct modification of the tropical forest's structure, composition, and productivity.
>
> In spite of well-known ecological limitations for present-day humans dwelling in tropical forest environments (animal and plant-food supplies are

> highly diverse, dispersed, and difficult to obtain, but see …[eight references from 1990-1991 –ED],) this volume presents archaeological evidence that the occupation of tropical forests has deep roots and much predates the horticulturalist colonization of these ecosystems. An early pre-farming settlement of tropical forests is the rule, not the exception. The archaeological sequences reported in this book, as well as those reported elsewhere … [four references from 1987 to 2000 –ED], demonstrate that tropical forest environments supported a continuous settlement by hunter-gatherer groups for millennia.

If, however, one will recall the frequent allusions to the period of **hyper-aridity** in Africa that one is confronted with from time to time, then there are grounds for arguing that the forests were **not** eternal, that they were prone to disappear during periods of high **glaciation** in Europe which correlate with the hyper-aridity in Africa. Or as Julio Mercader put the question (on page 3):

But, Was There Any Forest at the Time of Pleistocene Occupation?

His answer was, as follows:

> An assessment of the feasibility of tropical forest occupation by prehistoric hunter-gatherers relies on the available environmental data to demonstrate the timing, geographical distribution, and nature of lowland forest formations in the distant past… Until the late 1980s archaeological inquiry on the ability of humans to occupy lowland tropical rain forests was highly dependent on biogeographic models derived from the **'refugia hypothesis'**… ., geological indicators of perceived 'aridity'…and paleoenvironmental records from forest-fringing sites and regions separated from the lowland forest by large geographical distances...
>
> Current data suggest that during glacial episodes of the late Pleistocene the wet tropics may have sustained heterogeneous vegetational formations, including tropical forests that subsisted in a cooler, drier, and CO_2-starved atmosphere and yielded admixtures of highland and lowland species with many shrubs and herbaceous plants on the forest floor. Recent data worldwide suggest that, for the late Pleistocene at least,
>
> 1. Some tropical lowlands currently covered by evergreen forest were not severely deforested, as shown by the presence of arboreal taxa in the pollen and phytolith records older than 10,000 B.P. Botanical assemblages indicate a lowering of montane altitudinal vegetation belts and an admixture of lowland and highland species …[thirteen references for Africa, Amazonia, Southeast Asia –ED]
> 2. There was an overall drop of temperature of approximately 5-7°C… Therefore, cooling environments are expected during glacial periods, thereof called 'hypothermals'.
> 3. CO_2 content in the atmosphere could have been remarkably lower, causing significant changes in plant development, forest structure, and altitudinal distribution of plants … [two references –ED]
> 4. Rainfall may have decreased 20-30% … [two references –ED]. Yet, sites in wet environments receiving approximately 2000 mm to more than 3000 mm of annual rainfall did not undergo severe deforestation because even under a

highly unlikely 50% reduction in rainfall, their water balance would be enough to support forests. The changes in the evergreen forest involved species reassembly resulting from downward displacement of montane elements and/or invasions of trees characteristic of seasonal formations … [two references –ED], rather than simple cycles of contraction and expansion derived from the climatic aridity predicted by the refugia theory. On the other hand, the drier types of forests could have interdigitated with sclerophyll taxa to variable degrees … [three references –ED].

5. During cooler and drier periods of the Pleistocene, the open forest structures that appeared in some regions yielded a large supply of economically exploitable taxa in the form of undergrowth, improving food supply and overall suitability for occupance by early humans.

Editorial helpful comments: since forestry is the main specialty of so few of us, it may be useful to spell out more fully a few of Mercader's points. Thus:
a) good to remember that standard classic tropical rain forest lacks dense 'undergrowth', all those grabby thorn-bearing bushes and shrubs the explorers have to hack their way through in standard Hollywood jungle movies. The reason simply is lack of sunshine getting down to the forest floor. So 'open forest' in Julio's terms means a forest with gaps in the canopy which foster the growth of 'undergrowth' i.e., a variety of bushes, et al, some of which are good to eat.
b) a place with 50% of 3000 mm of rainfall would be getting around 38 or 39 inches of rain per year, about the same as the northeastern USA or central Ethiopia.
c) Referring the reader to 'evergreen forest', i.e., tropical rain forest, is apt to confuse someone from the northern latitudes, like New Hampshire or Latvia, where the evergreens usually are soft wooded trees. The tropical rain forests have some of the hardest woods in the world, e.g., ebony which is no Tannenbaum.
d) Those who held that man only began living in the rain forests after farming opened up the way may have influenced the viewpoint of the 'coastal hypothesis' which we discussed earlier. First in southern India and later in southeast Asia the first colonists allegedly stuck to the coastal areas rather than venturing into the deep woods, presumably because they could not cope with the tropical rain forest. The coastal hypothesis does **not** need this corollary – colonists could have entered the forests after all – and here may be the reason the hypothesizers apparently saw the forests as barriers.

Such a valuable book as *Under the Canopy* is fundamentally a reference book and cannot be adequately summed up in the space we have alloted to it. Its key points have already been mentioned; it remains to put some dates together with specific areas in order to see Julio Mercader's thesis in detail Hereafter we follow the Table of Contents, sticking dates on specific areas when available. (Julio's **Introduction** precedes these chapters.)

Part I

African Pioneers

The Archaeology of West Africa from the Pleistocene to the Mid-Holocene

By Joanna Casey. Pages 35-63.

The Middle Stone Age Occupation of Atlantic Central Africa: New Evidence from Equatorial Guinea and Cameroon
By Julio Mercader and Raquel Martí. Pages 64-92

Foragers of the Congo: The Early Settlement of the Ituri Forest
By Julio Mercader. Pages 93-118.

Part II
Australasian Settlers

Hunter-Gatherer Occupation of the Malay Peninsula from the Ice Age to the Iron Age
By F.David Bulbeck. Pages 119-160

More Than a Million Years of Human Occupation in Insular Southeast Asia: The Early Archaeology of Eastern and Central Java
By François Sémah, Anne-Marie Sémah, and Truman Simanjuntak. 161-190.

An Archaeological Assessment of Rain Forest Occupation in Northeast Queensland, Australia
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Part III
The Last Frontier: Newcomers in a New World

Late Glacial and Early Holocene Occupation of Central American Tropical Forests
By Anthony J. Ranere and Richard G. Cooke Pages 219-248

Holocene Climate and Human Occupation in the Orinoco
By William P. Barse Pages 249-270

Archaeological Hunter-Gatherers in Tropical Forests: A View from Columbia.
By Santiago Mora and Cristóbal Gnecco. Pages 271-290

Hunter-Gatherers in Amazonia during the Pleistocene-Holocene Transition
By Betty J. Meggers and Eurico Th. Miller. Pages 291-316

In order to be **fair** in this test the **dates** must be for human debris (artifacts or fossils) which are associated with tropical forest debris or indications of such. We have, for example, in Central America human remains from the highlands of Guatemala –not presently forested – but which **were** tropical forest at the time of deposition of the human remains (10,000 BP or so) There are a large number of sites which show dates associated with forest conditions but with no human debris. They can testify as to earlier climates but not for human occupation of forests.

With those guide lines we may resume sifting through the site reports.

First questions first: when did mankind (hominids) **start** living in tropical rain forests? In the Miocene to Pliocene (6 mya to 3 mya) earliest biped hominids in Africa were found in a wide range of habitats but it was not clear that lowland rain forest was one of them or the most common one. So Julio's answer is basically **maybe.** Of course

there was an old assumption that **'man had come down out of the trees'** in the early stages of human evolution. In the time of *Homo erectus* their major focus was on southeast Asia with all that makes likely but at one site at least (**Bose** in southern China) the palaeoenvironment was not a rain forest.. This led Mercader to conclude that the tropical rain forests of southeast Asia, even possibly including one site on Flores, could not be excluded from *Homo erectus* habitats. I take this response to be **'probably but not certainly'** *Homo erectus* lived in the lowland rain forests of southeast Asia, at least part of the time. Then, during the last 300 kyr, one of the dominant varieties of Homo, namely *Neanderthal,* never lived in tropical rain forests for obvious geographical reasons. The other variety, or *Homo sapiens*, is the controversial one. McBrearty and Brooks (2000) believed that it was only anatomically modern humans, and almost necessarily the Middle Stone Age folk, who were strong enough and clever enough to enter the 'extreme environment' that was the lowland rain forest of the tropics. The cleverness was inferred from the tool kit, as we have discussed before, a not unnatural assumption.
As a clear statement of his thinking, Mercader said :

> "We do not know when it was that our ancestors first came into the rain forest, **if they ever left it**, but, whether *Homo sapiens* was the first colonizer of rain forests or not, current archaeological evidence indicates that before 40,000 B.P. modern humans had already encountered, crossed, and settled tropical forests of all types during their expansion across the vast expanses of land that go from Africa to Australia." [Emphasis added –ED]

Why would he peg the key date at 40,000? Probably with Klein's Aurignacians in mind, he wanted to establish that humans had spread around the Old World before their departure from Africa.

Nevertheless, Julio established human cultures (Middle Stone Age) in the rain forests of Ghana, Gabon, Equatorial Guinea, Cameroon, and Congo; as early as 255 kya for 'Sangoan' and 95 kya in Côte d'Ivoire, another at 100 kya and 65 kya in 'western East Africa' and as late as 35 kya in Ghana and Cameroon. Other later human cultures of the Later Stone Age type included foragers in the northeastern Congo by 20,000 B.P. This is where we find the Mbuti Pygmies today. The earliest LSA (Later Stone Age) site so far in the lowland rain forest is in Gabon at 40,000 B.P.

Human cultures were also present in Southeast Asia (specifically Borneo, Malaysia, and Philippines) 'before, during, and after the last glacial maximum' which is getting a little vague but probably means from 25 kya to less than 10 kya. They also reached and settled most of the Sahul out to New Guinea and Australia and some of the Melanesian islands. Except for the bulk of Australia, most of this area was tropical rain forest.

Even though he is quite sophisticated scientifically, Julio Mercader is too chary and diffident about his conclusions. Although he has clearly falsified two important archeological hypotheses which existed more as ideology than theories, he is loathe to say so directly and forcefully. So I will: **the notion that the tropical rain forest qua environment is too extreme, too stressful, for human habitation short of the agricultural revolution is FALSE.** The falsification was quite easy; colleagues simply looked into the matter empirically. That is just ordinary standard science, not some elaborate judgmental affair.

The second notion that has dominated recent discussions, and one which Julio seems to believe himself, is that since the **tool kit** reflects adaptation to some eco-system, i.e., **behavior,** it also reflects cognitive skill, i.e., **intelligence** or the prevailing level of intelligence. But it seems to have two corollaries, thus: the tool kit is accurately reflected in the **stone** tools available at a given site and the tool kit accurately reflects the **technology** present in the culture in question. As a cultural term or a concept found in cultural theory, the word TECHNOLOGY does have somewhat different meanings or interpretations to various theorists. I would offer this one which aims to find the least common denominator among definitions. Technology mostly means **all the knowledge** and **all the skills** available to a society (people) in their interactions with physical reality, the natural world, and/or other peoples. One uses technology to build a boat, find something to eat, start a fire or fight an enemy. Among other things. One could do most of these things wiithout any stone tools, except that it would be more difficult to build a boat without something like an axe.

Having established that human cultures lived in the lowland rain forests from at least Middle Stone Age time onward, Mercader notes that the stone tool kits of the foresters are generally the same as those of the savannah dwellers. There seems to be nothing special about the forest tool kits as opposed to the savannah tool kits; indeed the latter are the models of the tool kits. Since the tool kits are the **same**, there seems to be no **behavioral** difference between forester and plainsman, just as there **is** a behavioral difference between Middle Stone Age culture and Later Stone Age culture (or candidates for being called 'Aurignacian'). But put the matter in terms of technology, the source of adaptive behaviors, and the proposition is **obviously false**. One could see this immediately by looking at the **four pages** of lists of valuable plants which can be found, or could be found, in the African lowland rain forests. Julio lists 'fat calories/oil extraction, starch consumption, vegetable, seeds and fruits, spices, and stimulants' and the botanical names of the plants with these qualities. As any Khoisan-speaking female could tell you, this is technology I carry in my head or I can get by asking a kinswoman and I use it to feed me and my family' Or as Julio argues – this technology was the key to successful adaptation to the forest eco-system

As has been pointed out by G. Pope (I believe), and many others informally, man's adaptation to Southeast Asia, including south China, cannot be understood without reference to a certain kind of plant, called BAMBOO. So many things are made of bamboo that simply concentrating on the stone tool kits will **not** lead one to comprehension of the local technology A lot of that exists in Africa. And not only the Pygmies and other hunter-gatherers. I once asked the Ganjule of Lake Chamo in the southern Rift Valley of Ethiopia how they killed the large dangerous hippos in the lake (which they did for the meat). Did they use rifles or spears with iron points, which they had, for hunting hippos? No, they killed them with large long wooden spears with fire-hardened points (no iron) and they snuck up on the hippos in the early morning. I was near there when an angry hippo killed or wounded 20 people after he had been angered by attempts to shoot him with rifles.

(Ganjule bravery is not normally called part of their technology, but their wariness in front of crocodiles would have to be called part of their religion. Yes, they venerated crocodiles and would not kill them when the crocs invaded their hamlets! No, they didn't let the crocs eat Ganjule children. So they yelled at and threw sticks and stones at the crocs. It worked. Crocodile god re-entered the lake.)

Chapter 1. Joanna Casey. West Africa generally (Chad to Senegal) harder to analyse because of incompetent earlier work and harder to date beyond the C^{14} limits (40 ± kya) because volcanic activity is lacking. Some indications that southern West Africa may have been settled by humans **much later** than East Africa are partially offset by some sites in Ghana which put such habitation earlier than thought. Some avoidance of rain forest area by archeologists frustrates the basic question.

Chapter 2. Mercader and Marti. Cameroon and Equatorial Guinea. Although some **Acheulian** type tools have been found in the forest in the Central African Republic, the main evidence comes from the Middle Stone Age. Taking the forms of the somewhat debated '**Sangoan**' and mostly later '**Lupemban**' style 'techno-complexes', basically MSA industries were spread across most of middle Africa, including forest areas, in the uncertain sites such as those in Chapter 1. However, in two sites in the heart of hearts of rain forest Africa MSA & LSA tools were dated to as early as **35+ kya** (Njuinye) in Cameroon and **30 kya** (Mosumu) in Equatorial Guinea (formerly Spanish Guinea). Njuinye was a deep stratified site with tool deposits from 16 cm to 437 cm at bottom (more than 14 feet down). Mosumu included some Lupemban points ('large bifacial lanceolates'), as did Njuinye. More relevant is the authors' conclusion that "This region supported a **continuous human occupation** by both pre-LSA and LSA humans throughout the late Pleistocene and the entire Holocene…" On pages 82-83 they list more than 30 different MSA and LSA sites with their locations and dates in central Africa. About one third are forest sites, another third savannah, and the remainder so-called 'mosaic'. Dates range from 35 kya to one millennium for forest sites; one savannah site in the Congo was 44± kya (P. de la Gombe). We are reminded that some contemporary **savannah** areas were in fact rain forests during some 'wet and humid' phases (eras).

. Chapter 3. Mercader. The Ituri Forest of the Congo. More or less continuous settlement revealed in rock shelters from around 20,000 BP to recent times and in undoubted forest. Some particularly dense rain forest with virtually no undergrowth in the southern parts of Ituri show little evidence of human habitation, while somewhat more open canopied forest with undergrowth correlate with most of the occupied sites.

Evidently, Julio Mercader has **refuted the presumptions** about human unfriendly rain forest which has apparently dominated much of archeological thinking about tropical jungles in the past. However, he and his colleagues seem also to have modified the friendly forest viewpoint to stress the importance of **undergrowth** which is crucial to satisfying the **carbohydrate** needs of the human diet. This does, however, provoke another question: whence come the carbohydrates in the Polar Eskimo diet – other than animal fats (blubber, fish oils, etc.)??

Yet, contrary to Julio Mercader, the archeological presumption of unfriendly rain forests was not shared by some major ethnologists. For example, G.P.Murdock in his discussion of southern Nigeria, Cameroons, and the Congo (in his *AFRICA* book, 1959) talked about the 'Sangoan' archeological associations with hunter-gatherer Pygmies who lived in the great forests long before agriculture. Biasutti and colleagues in their huge Africa book of 1955 asserted that:

> "I Pigmei sono gli aborigeni delle immense foreste equatoriali, dall'Atlantico al Tanganica. Le loro tradizioni sono concordi con quelle dei popoli negri nell'asserire che quando questi ultimi giunsero sulla terra vi trovarano gia i piccoli cacciatori della foresta. Altre considerazioni lo confermano: l'assai maggiore primitività della cultura pigmea, il perfetto adattamento dei Pigmei allo speciale ambiente (adattamento che i Negri sono ancor offi lungi dall'avere raggiunto in ugual misura); la stessa distribuzione dei primi limitata oggi proprio alle zone antropo-geograficamente idonee a segregare e proteggere gruppi umani primitive. Il contrasto fra i due elementi etnici dell'Africa equatoriale è altrettato netto nel campo culturale quanto in quello razziale. Distribuiti in piccoli nuclei entro territori sterminati, che ovunque considerano come loro patria di origine e lore proprietatà, i Pigmei dipendono per l'esistenza dalle primordiali attività della raccolta e della caccia, ignorando ogni accumulazione di alimento o di beni materiali, vivendo da un giorno all'altro di quanto la natura offre..." (1955:561-562).

Indeed Dan McCall reports that an archeologist in good standing, one Merrick Poznansky, told him maybe twenty years ago that **yams** were grown in the rain forest as long ago as millets and sorghums were grown anywhere. Those would be **native** African yams and the dates would be several millennia before the time of Christ. In Somotic areas of southwestern Ethiopia, at least, people regularly gather **wild** forest yams to supplement their regular diets. In the same area **wild ensete** is known, along with the domesticated staple ensete. Both are most likely products of the forest, albeit montane in Ethiopia.

Chapter 4. Australasia. Malay Peninsula. F.David Bulbeck. Problems associated with glacial epochs are wholly different from those of Africa. In southeast Asia the emerging and drowning periodically of vast areas means that a large part of the evidence of the past now lies under water Along the core or backbone of the peninsula there were periods of rain forest (125-75 kya and 14 kya to present), partly rain forest (75-25 kya), and savannah or the local equivalent of 'mosaic' (25-14 kya). Sites were often abandoned for long periods of time as the sea retreated (or the land expanded!) and returned. At one bunch of sites (Bukit Jawa, Kampung Temelong, Lawin) **dates to 100± kya and 70+ kya** are estimated but they are not considered final or 'peer-reviewed' enough to be settled. The artifacts are large and crude, so the correlation with Homo sapiens – a crucial point in southeast Asia – is not sure. Remembering the problems with the artifacts of Flores, we shall be cautious but must report that the local archeologists seem not to doubt their human origin.

(If these dates turn out to be **true** and of *sapiens* origin, they in themselves are enough to **falsify** Klein's hypothesis. Their uncertainty has extended to television programs proclaiming their antiquity, as seen by at least one of our colleagues.)

At Lang Kamnan 'uncalibrated radiocarbon determinations' date a site with three primary strata with dates running from 31 kya to 7500 years, with a final reading of 150 years.

The last relevant thing about Bulbeck's rich article is what **burials** reveal -- a gradual **reduction in stature** of inland (or forest) populations is recorded for the Holocene where evidence of *Homo sapiens* is very clear. The coastal peoples did **not** shrink from something like a modern Malay norm of 5'5" but the forest dwellers – clearly ancestral to the **Semang** and probably the **Senoi** too – decreased to about 5' tall. The Semang are supposed to be the archtypes of the Negritos of southeast Asia, although they are a little taller than the Philippine Negritos. (Variations in hair form from curly to wavy to straight distinguish the Semang from the Senoi and others) Yet they are maybe 10 cm taller than the **Mbuti** Pygmies of the Ituri forest and still taller than the **Aka** (BiAka) to their north or the various **'Twa'** pigmoids to the east and southeast, both of whom are a few cm taller than the Mbuti.

Archeologically or historically, we have very few cases documenting the shrinking or the enlarging of forest or island populations. There is Sicily (shrinking), Malaya (shrinking), Polynesia? (enlarging) and where else? Mostly we have correlations between sizes and types of environment. Historical progressions are what we need. Boas once found that European immigrants to the United States in the late 19th and early 20th centuries got large and altered their cephalic indexes. The reason? Perhaps the political corruption rampant in our great Atlantic sea ports?

(Carleton Coon's book *The Living Races of Man (1965)*, although much despised by many anthropologists, is useful and stimulating reading on this topic. That does not mean he is right, of course, but modern DNA research can test his many analyses and conclusions. His writing is clear and interesting)

Chapter 5. Insular Southeast Asia, especially Java. François Sémah et al.

Java is unique in showing more than a **million** years of probable human habitation, making the inhabitants the "oldest islanders in the world". So we can answer one question we did **not** ask – did any of our relatives live in the rain forest for long periods of time? Since none of them practised agriculture, the answer would be clear. And Java's answer is clear – *Homo erectus* lived there a long time and much of it was spent in rain forests. So when did modern humans reach Java and did they live in rain forests?

Remember that during periods of high glaciation & low water Java was connected to Borneo, Sumatra and Malaya; thus it was part of an area as big as the Sudan. Much of that is now under water.

At a Holocene site in the Punung area (Song Keplek) a **"mousteroid"** industry was found in association with *Homo sapiens* physical remains. However, elsewhere in the Punung area the authors state that: "*Homo sapiens* occupied the cave sites of the Punung area around 45,000 years ago and continued to inhabit the changing environment of central and eastern Java during the entire Holocene." Since there had been *Homo erectus* sites in the same area and close in time, it is likely that modern humans met their *erectus* cousins in Java at least. And, since eastern Java is the drier part of the island, the odds are that it was open forest or mosaic, rather than rain forest.

Chapter 6. Australia, **Northeast Queensland**. B. Asmussen. Since a major part of Australia is desert nowadays, and the southeastern coastal fringe is 'Mediterranean', one finds rain forest only in a few places. In relation to the rest of the continent the rain forest is somewhat analogous to Florida in its extent. Also nearly half of **Tasmania** is covered by rain forest. Generally these Queensland rain forests are somewhat different in that they are rather more open, the canopy is not as high, sunlight gets in more, and there is undergrowth, specifically carbohydrate rich plants and tubers.

The author believes that most archeologists in Australia reckon that Homo sapiens arrived in Australia from the north about 50,000 years ago. [It seems that you get a different date for each archeologist –ED] A large number of sites are known in northeast Queensland but most of them have not been excavated, as yet. There are also ethnographic reports of tribes living in the rain forest and living richly as hunter-gatherers because of the abundance of undergrowth **nuts**, for which their tool kit included **nut-cracker stones and grinding stones.** Finally Bulbeck sums up:

> ..When was the rain forest iniially occupied? The only radiocarbon dates available to answer this question date back to the mid-Holocene and were collected from the basal strata at one site, but do not represent the initial settlement of the whole Australian forest. Likewise, a late Pleistocene antiquity is indeed possible but it remains to be shown with direct evidence. Ecological data, for their part, suggest that in the past the Australian forest has been heterogeneous and very productive, undermining the claims of unity and marginality of this type of environment. Nonetheless, new paleoenvironmental data are required to evaluate resource availability, the impact of climate change, and human intervention in these patchy ecosystems."

Chapter 7. Central America. Ranere & Cooke. Their initial summary is adequate and to the point, a follows:

> "The focus of archaeological research in Central America, like other tropical regions of the world, has not been on early hunter-gatherer occupants of tropical forests but rather on farmers and village dwellers living in anthropogenically modified habitats. Nevertheleess, a gradual accumulation of data, with some acceleration of late, has made it quite clear that human populations were living in Central American forests at least by the late glacial stage (circa 14,000-10,500 B.P.) of the last glaciation … and continuously thereafter, initially as hunter-gatherers and later as agriculturists."

Their Central America is in fact 'Meso-America' to other anthropologists because they include Mexico. Even so, traditional Central America (Guatemala, Honduras, Belize, El Salvador, Nicaragua, Costa Rica and Panama) is as big as Sumatra or the Northeastern states of the U.S.A. (Pittsburgh to Fort Kent, Maine), even if Mexico is four times as big.

Clovis or clovis type tools are characteristic of the early sites in Central America even though the authors say that pre-Clovis peoples and tool kits are possible and may yet be foumd. The Clovis Line is the famous determination by dominant American archeologists that there were no humans before that time.

Chapter 8. South America. Orinoco river valley, Columbia-Venezuela border. W.P.Barse. In a report concentrating on the pre-ceramic horizons Barse talks about levels dated to the Holocene and mostly 9000-7000 years ago. The tool kits are all about flakes and river stones reduced to cores and the like. There is no mention of Clovis type fluted points, although at a later period there are points which remind me of arrowheads.

Barse in his conclusions tries to throw light on the disagreement between Roosevelt and the Clovis school, as follows:

> "If the above noted interpretations from the pollen record and its correlation with early Holocene paleosols are accurate, then the initial Archaic occupants of the Orinoco valley were adapted to tropical forest conditions and not the open

> savanna-gallery forest mosaic that characterizes the region today. The impact of the dry-wet cycles throughout the Holocene (with an increasing focus on drier conditions or shorter rainy seasons) on human populations adapted to the lowland environment needs to be considered to enhance an understanding of the development of tropical forest culture…"
>
> "…For instance, to date, there is no evidence for a Paleoindian (or perhaps more appropriately, a late Pleistocene occupation) in the Orinoco Valley. This apparent gap may be nothing more than a lack of an adequate survey of the region. The archeological signature of small, transient camps dating to this time is undoubtedly small and may take extensive surveying and testing to detect. Whether or not early Holocene Archaic-stage occupations are the earliest in the Orinoco region is still an issue that needs to be considered. Roosevelt et al's (1996) contention of a separate Paleoindian occupation of Amazonia separate from a ``Clovis-derived base has been disputed … (3 references, including himself). The early Holocene occupations that she reported on from Pedra Pintada are more in line with the early Holocene Archaic components discussed above for the Orinoco."
>
> "One characteristic of Archaic assemblages in the Orinoco and other northern lowland sites is the generalized nature of the lithic assemblages, dominated mostly by unifacial flake tools and an occasional ground stone tool, makes comparison between regions difficult at best. However, they are widespread, occurring in late Pleistocene to early Holocene sites in the Sabana de Bogotá east to the Orinoco and south in the Brazilian Shield area. One question is whether or not such nondescript lithic assemblaces mark adaptations to forested environments. Were such assemblages nothing more than **expedient** tools used to fabricate an assemblage from perishable materials? …"

[perhaps he wanted to say 'wood'? –ED] One should note that Roosevelt's site was much **older** than his Orinoco sites

Chapter 9. South America. Columbia. Mora and Gnecco. At the site of Peña Roja whose earliest inhabitants arrived around **9200** BP:

> "the stone industry produced by early inhabitants of the site consists of unifacial industries with little or no retouch. Chert is the main raw material. Tools include concave scrapers on thick flakes, wedges, notched flakes, and perforators. But unretouched flakes form the bulk of the artifactual evidence."

At the site of San Isidro the authors got three earlier dates of 9530, 10,050, and 10,030 B.P. and had this to say in their Discussion:

> "C.Levi-Strauss (1950) noted that farming societies of South America complemented their farming economies with wild resources …(1 reference). If this was the case among ethnographic farmers, it is reasonable to assume that it was more the case with prehistoric hunter-gatherers with mixed forager-farmer economies. We believe that late Pleistocene and early Holocene hunter-gatherers were efficient managers of tropical forests and enhanced the natural productivity of their resource base. South American tropical forest foragers hunted, gathered, and, somehow, produced their resources. Binford (1980) suggested that residential mobility among hunter-gatherers was almost exclusively contingent

> upon resource distribution. Yet, mobility was determined by many factors. If residential mobility among Peña Roja and San Isidro foragers influenced local resource distribution through intentional manipulation, then access to resources may not have been free for all individuals from all groups. Late Pleistocene to Holocene Amazonian foragers could have regulated access to resources and exercised some kind of territoriality…(1 reference). It is possible that various forms of low residential mobility and territorial societies could have evolved in neighboring Andean societies since the late Pleistocene… (2 references). R.C.Bailey and T.N.Headland (1991) predicted that if foragers were living in tropical rain forests before the introduction of agriculture, they would have had to be more mobile than Pygmies, Agta, Batek, and Punan groups are today. We do not believe this holds true for the Columbian instance 10,000 years ago. Mobility is determined by the type of exploitation and control over local resources."

Chapter 10. South America. Amazonia, in general. Meggars & Miller.
The authors cite several sites as early as 27,000 BP and many more in the 10-13 kya range, including Anna Roosevelt's Amazonian dig. Three rock shelters in Brazilian Amazonia are Pleistocene sites within the range of the general pattern of the rain forest but not necessarily rain forest at the time of occupation. Their dates, of course, are obviously **much older than the Clovis horizon in the United States** and in this instance it does not matter so much if they are precisely in the rain forest whose extent varied considerably during the Pleistocene. The sites are Caverna da Pedra Pintada on the left bank of the lower Amazon; Abrigo do Sol and Santa Elina in the southern realm of the Amazonian rain forest. Caverna de Pedra Pintada was Roosevelt's site, dated to 10-11 kya on the basis of 56 radio carbon dates (readings). As Rooevelt pointed out, this makes the Caverna dates roughly contemporaneous with Clovis sites in the USA. At Abrigo do Sol thirty-two radio carbon dates range from 14,700 to 5760 BP, although an isolated date of 19,400 was obtained. Called 'problematic' by the authors, it was nevertheless compared to the dates from Santa Elina to its east. At Sanata Elina the earliest 'assemblage consisted of unifacial flakes, associated with Glossotherium remains. They were dated by Uranium-Thorium to 27,000 BP ± 2000 years. A "second assemblage, characterized by modern fauna, lithics [stone tools –ED], and hematite fragments, is carbon-14 dated between circa 13,000 and 7000 B.P. This and 40 other rock shelters in the region contain **rock art** of uncertain antiquity".

It is dubious that such an array of dates as these can be **doubted out of existence**, as was done to Scotty MacNeish's site in New Mexico by the Clovis horizon defenders. Since dates are so important, we will rest Julio's case here; although we recommend that interested parties read the rest of Meggars and Miller's article; they bring ethographic, botanical and linguistic evidence to bear on the whole problem of life in Amazonia before agriculture and at its beginnings.

Conclusion of this discussion of Julio Mercader's book

After a long and substantive discussion by Julio Mercader and his colleagues about humanity's interactions with the great tropical rain forests during the many many millennia spent hunting and gathering it is clear that the '**null hypothesis**' is **false.** The issue was not even close. We also learned more interesting things about specific conditions of the forests and their relationships to food supplies. But perhaps most interesting of all is the apparent relationship between tropical rain forests and the size of the individual humans living in them. Those ostensibly **long** in residence are **smaller** than populations outside of the forest. Without reviewing the evidence all over again, let us halt here and declare that this is the rough outline of a **new hypothesis** and one which deserves to be fully tested. (This hypothesis has precedents in the literature of biological anthropology.)

A poem most appropriate in its feelings about forests was presented at the Celebration of the Life of Joseph H. Greenberg at the Stanford Church on October 23, 2001. It was apparently read by Joe Greenberg in the Spring of 1932 when he graduated from James Madison High School in Brooklyn, New York. We do not know if Greenberg was the poet or not, or if he even was the reader. But it makes its point.

I traveled once through woodland dark and gnarled
With vast misshapen trunks and twisted boughs;
        And weary utterly of toil and strife
I laid me down in dreamless sleep, where time
Was but a vague and nameless fantasy, and earth
Breathed into every limb new life and joy.
The soft and lute-like harmony of birds
Awoke me and the smiling sun that shone
Gave benediction. Everywhere was light,
And all the gaunt, forbidding shapes had fled.

[Thanks to Merritt Ruhlen for sending me a copy of the service program –ED]

Chinese Geneticists Report on China's (human) Genome

It is not the case that a report on the genetics of China should wait for seven years to be published in *MOTHER TONGUE*. Far from it! And even less excuse when the report was made by Chinese scientists, instead of foreigners. There seem to be two basic reasons why our report has been delayed. The publication in the *PNAS* (*Proceedings of the National Academy of Sciences)* virtually guaranteed that it would be way down the list of journals perused for current developments. Only recently have we realized its importance. After having followed geneticists very closely in the 1980s and 1990s, we various editors had paid more attention to other things more recently. And finally last year the Chinese report got 'bumped' because of space demands.

The report was entitled **Genetic relationship of populations in China** and it showed up in the *PNAS*, vol. 95, pp.11763-11768 in September of 1998. The authors were **J.Y.Chu, W.Huang, S.O.Kuang, J.J.Xu, Z.T.Chu, Z.Q.Yang, K.Q.Lin, P.Li, M.Wu, Z.C.Geng, C.C.Tan, R.F.Du, and L.Jin** who will be referred to from here on out as **Chu et al**. As is the custom in some publications, this article was accompanied by another article about it in the same issue of PNAS (pp.11501-11503) written by L.Luca Cavalli-Sforza, entitled "*The Chinese Human Genome Diversity Project*". Since there is no need to reduplicate the comments of fellow long ranger and professional geneticist, Luca Cavalli-Sforza, we leave that to our members to follow up themselves. The Abstract of Chu et al is, as follows:

> Despite the fact that the continuity of morphology of fossil specimens of modern humans found in China has repeatedly challenged the Out-of-Africa hypothesis, Chinese populations are underrepresented in genetic studies. Genetic profiles of 28 populations sampled in China supported the distinction between southern and northern populations, while the latter are biphyletic. Linguistic boundaries are often transgressed across language families studied, reflecting substantial gene flow between populations. Nevertheless, genetic evidence does not support an independent origin of *Homo sapiens* in China. The phylogeny also suggested that it is more likely that ancestors of the populations currently residing in East Asia entered from Southeast Asia.

A less technical description of the genetic procedures was given by Luca on page 11501, as follows:

> "Microsatellites are repeats of short DNA segments, practically less than five nucleotides long. They have a high mutation rate and therefore a large number of alleles, which makes them perhaps three times more informative on average than the most common type of genetic polymorphisms, single nucleotides substitutions, which are mostly biallelic. They are used very widely in genetic linkage studies and have begun to be used in evolutionary analyses. ..Thirty microsatellites were tested by Chu etal for reconstructing a tree of 14 East Asian populations, which were studied along with 11 populations of a standard set representing the rest of the world. A subset of 15 of the same microsatellites were used to construct a second tree from 32 East Asian populations. These include the first 14 and are compared with the same 11 populations from the rest of the world."

Aside from the technicalities of genetic labors, the article is all about taxonomy and prehistory. Here first is the list of the 28 populations sampled in Chu et al, as they are listed along with their locations and linguistic affiliations:

Population	Location	Language Family & Sub-Family	
1. Aini	Southwest Yunnan	Sino-Tibetan	Tibeto-Burman
2. Blang	Southwest Yunnan	Austroasiatic	Mon-Khmer
3. Dai	South Central Yunnan	Daic	Daic
4. Deang	Southwest Yunnan	Sino-Tibetan	Tibeto-Burman
5. Dong	Guangxi	Daic	Kam-Sui
6. Ewenki	Heilongjiang	Altaic	Tungus
7. Han (Guangdong)	California, USA	Sino-Tibetan	Chinese
8. Han (Henan)	Henan	“ “	“
9. Han (northern)	Beijing	“ “	“
10. Han (Yunnan)	Yunnan	“ “	“
11. Hui (Muslims)	Ningxia	Sino-Tietan	Chinese
12. Jingpo	Western Yunnan	Sino-Tibetan	Tibeto-Burman
13. Korean	Jilin	Isolate	
14. Lahu	Southwest Yunnan	Sino-Tibetan	Tibeto-Burman
15. Li	Hainan	Daic	Kadai
16. Manchu	Heilongjiang	Altaic	Tungus
17. She	Fujian	Hmong-Mien	Ho Nte [sic-ED]
18. Tibetan	Tibet	Sino-Tibetan	Tibeto-Burman
19. Tujia	Hunan	Sino-Tibetan	Tibeto-Burman
20. Uyghur	Xinjiang	Altaic	Turkic
21. Wa	Southwest Yunnan	Austro-Asiatic	Mon-Khmer
22. Yao (Puno)	Guizhou	Hmong-Mien	Hmongic
23. Yao (Jinxin)	Guangxi	Daic	Kam-Sui
24. Yi	Sichuan	Sino-Tibetan	Tibeto-Burman
Taiwan Aborigines			
25. Ami	Taiwan	Austronesian	Formosan
26. Atayal	“	“	“
27. Paiwan	“	“	“
28. Yami	Lanyu	“	Malayo-Polynesian

Not all of the language names or population names are familiar to ASLIPers. Nor are the taxonomic units quite the same as those we might use. Probably the most serious differences pertain to the Formosan and Korean cases. Neither Blust, who owns the dominant modern classification, nor Ruhlen (in his *GUIDE*) would agree with ‘Formosan’ being a sub-family of Austronesian. Both Atayal and Paiwan diverge so much from each other that each has a sub-phylum of Austronesian named after them, Atayalic and Paiwanic respectively. Ami is a segment of Atayalic, while Yami represents another sub-phylum, Malayo-Polynesian with its hundreds of members. Korean of course has not been an isolate for years now, being related (perhaps against its will) to Japanese, and less closely to Altaic (especially Turkic and Tungusic) and to Gilyak.

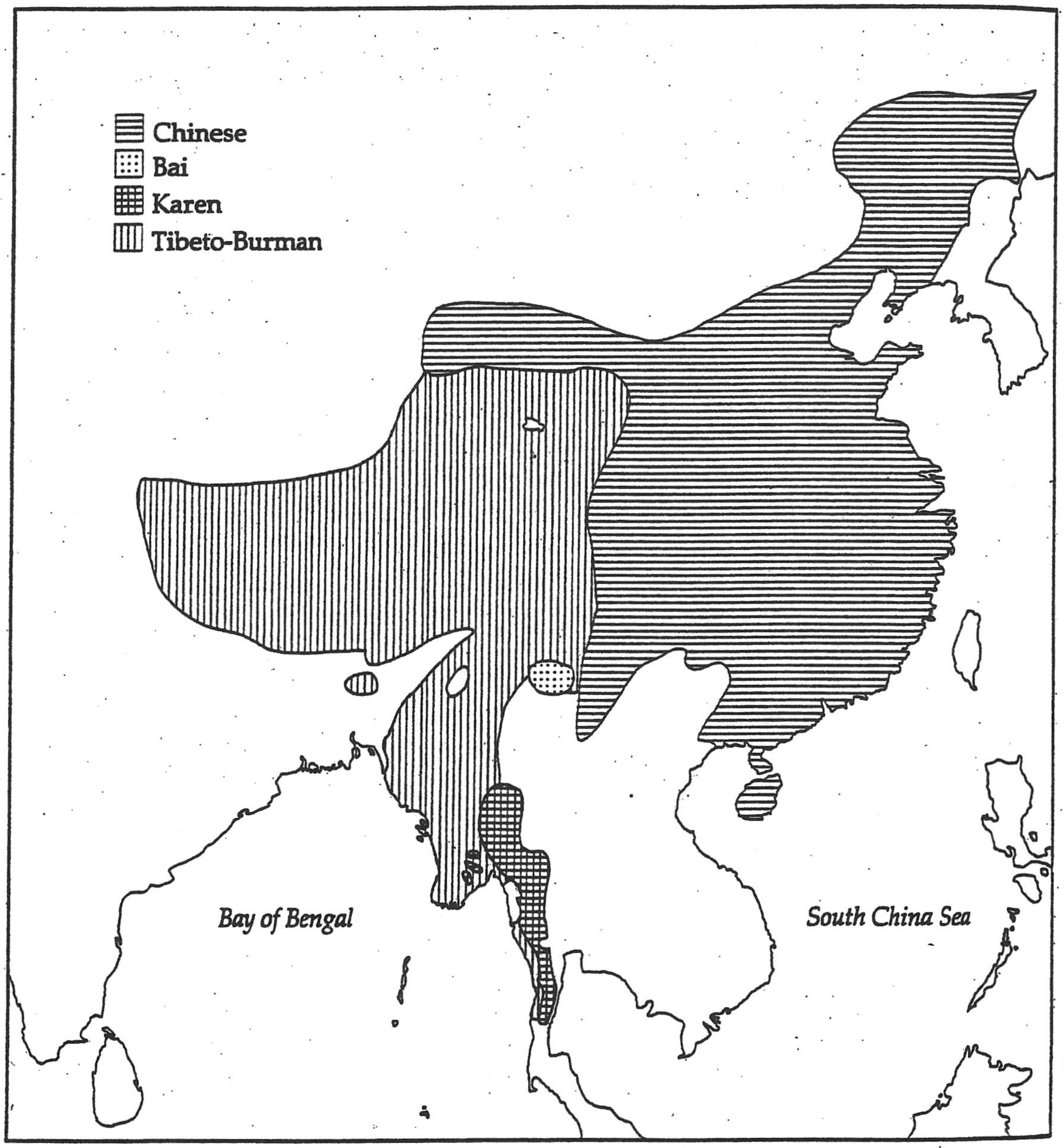
As Luca points out, one ethnic group, Han Chinese, accounts for 1,100,000,000 people; the other fifty-six ethnic groups account for 100,000,000 people or an average of one and three quarters million per ethnic group. By the standards of Austronesia or New Guinea those are hefty ethnic groups and probably of a size to maintain themselves in the face of the overwhelming social predominance of the Han Chinese.

For the purposes of their research goals Chu et al selected a 50% sample of the 56 ethnic groups which must have included Taiwan's 15 (not including Han Chinese). Note that these were not population samples but rather, and basically it appears, samples drawn from populations speaking one of the various ethnic languages or a regional population of Han Chinese. So 28 ethnolinguistic groups from China were sampled, including some from alleged parts of China like Tibet, Sinkiang (Xinjiang), and Taiwan. Since China is an old conquest state, on a par with the Russian empire, the United States, Mexico or Brazil, the question arises: why sample non-Chinese areas like Tibet or Sinkiang but not Burma, Thailand, Malaysia, Laos, Vietnam or Himlayan India or Nepal where most of the relatives of the non-Chinese ethnolinguistic groups of China live?

For example, in the Sino-Tibetan phylum itself which was represented 12 times in Chu et al's sample (5 of them Han Chinese) there exist 258 languages about 246 of whom live in India, Burma and Thailand or Chinese Tibet. All this according to Ruhlen's GUIDE (pp.331-333). Of the 8 Chinese languages ('dialects') registered as divisions of Sinitic, about 62% of them were represented, albeit under the rubric of 'Han'. Phylogenetically, however, half of Sinitic was ignored, since the 4 **Bai (Minchia)** languages were not sampled. The equivalent in Italic of Indo-European would be to sample five Italian 'dialects' from Milan to Palermo but to ignore Sardinian. So we can say that Sinitic was imperfectly represented, but with 43% (12/28) of the total sample coming from Sino-Tibetan populations that phylum was not badly represented.

Most scholars follow the order of relationships for Sino-Tibetan, as outlined by Ruhlen's GUIDE; that is that Sinitic is one moiety of the phylum and Tibeto-Burman is the other, containing major phratries (sub-classes) of Tibetic, Burmic and Karen. Although I cannot identify the Tibeto-Burman languages listed by Chu et al as **Aini, Deang, Tujia**, or **Yi,** the presence of **Jingpo (Jinghpaw)** and **Lahu** of Burmic plus **Tibetan** of Tibetic suggest a rough balance of representatives. Since three languages (Aini, Deang, Lahu) come from 'Southwest Yunnan' they are more likely to be Burmic or Karen than Tibetic and Lahu is known to be Burmic. Yi of Sichuan is more likely to be Tibetic. Tujia in Hunan far to their east lies outside the usual range for even Karen or Burmic and is thus unpredictable.

However, if we follow George Van Driem's more recent re-classification of Sino-Tibetan, based primarily on grammatical evidence, Sinitic is de-throned as a moiety and becomes a sub-moiety, thus increasing the taxonomic importance of the 264 languages usually called Tibeto-Burman. Perhaps more telling is a look at Tibetic which is represented only by 'Tibetan" (probably the Standard of Lhasa). Yet there are 75 Tibetic languages overwhelmingly concentrated in the Himalayas or Tibet proper or western China; at least 21 are probably accessible in western or northwestern China. As one can readily see from Map 1 (overleaf)* it is difficult to visualize Sino-Tibetan as having a homeland in either northern China proper or in the eastern half of China. With Bai and Karen throwing their weight around, a homeland not far from western Yunnan or Sichuan (Szechwan) is not at all hard to imagine. Or in northern Burma. But this seems to be contrary to fact. At least for Chinese, the overwhelming weight of history, archeology, surname distribution, and genetics combine in pointing to a north Chinese expansion or migration across the Yangtze to the south as the principal peopling event of the past several millennia at least. In his commentary Luca testifies vigorously on this point. The mystery is, then, how do we account for the distributions of Bai, Tibetic, Burmic, and Karen?


▤ Chinese
▒ Bai
▦ Karen
▥ Tibeto-Burman
Bay of Bengal
South China Sea

Map 1 The Sino-Tibetan family

One may doubt that archeology or history can sort out Sino-Tibetan prehistory any time this century. Surname distributions are promising within one language or group of closely related languages which the Chinese 'dialects' really are. But across 246 languages with substantial differences and descent rules – patrilineal, matrilineal, ambilineal, and bilateral – success is not too likely. Genetics? Theoretically, genetics could give us a family tree of all the ethnic groups of China, Tibet, Burma and indeed of all of Southeast Asia.-- whether their languages are related or not. In fact physical anthropologists, our grandfathers **not** our brothers, did lump everybody in the region together as 'Mongoloids' or the 'Mongoloid race'. Of course the Mongoloids were quickly sub-divided into a northern and a southern variety. Some also reckoned that 'cold adaptation' was the most salient characteristic of the Mongoloids and therefore were able to find a homeland in the frozen north for them. Otherwise the human bodies gave us no workable clues to the **homeland of Sino-Tibetan**.

Fortunately, we have three linguisitc clues or groups of clues which will help us. First, we can calculate an accurate family tree of all the branches of the family/phylum. Luckily for us the scholars of Sino-Tibetan have done that for us; it is fairly stable, except that new languages keep getting added to the Tibetic branch, mostly in the Himalayas but also in the northern and western reaches of Tibet on the way to Sinkiang/Xinjiang. And also added to Burmic in the hill country near China and Thailand. Then we can calculate the most likely location from which all the daughters can most economically be derived. This process bears the label, Dispersal Theory. It failed dramatically in Austronesian by locating the homeland in Melanesia, while today's consensus locates it in Formosa People who usually don't know much about it simplify Dispersal Theory to the area of greatest diversity in any family or where the most branches are or the central focus of the distribution of branches. Sometimes it fails and sometimes it is successful, i.e., useful. But to be done correctly Dispersal Theory depends most crucially on internal taxonomy, the relative weights of the branches. It was Isidore Dyen's failure to appreciate the heavy sub-phyla on Formosa that led to his failure to locate Austronesia's homeland properly; it was not his use of Dispersal Theory that caused his downfall. This counter to his critics.

With these approaches one can get two results for proto-Indo-Hittite. On the simplistic view **southern Russia** – actually the **Black Sea** would be superior –would be a kind of center or focal point. On the grounds of proper Dispersal Theory **Anatolia** would be an excellent location because it is a moiety of Indo-Hittite. Yet we must look closely at the logic of this conclusion. Since Moiety **A** (Hittite and its kin) is located only in Anatolia, it is more likely to live in the homeland than Moiety **B** (the non-Hittites) which is spread over western Eurasia from Ireland to Sri Lanka, But hold on here! Why is **A** more likely to be in the homeland than **B**? They are **equal** in taxonomic **weight.** Yes, but in order to calculate the homeland of Moiety B which is all over the map one has to go through the same calculations we went through before in order to get 'southern Russia' as the homeland. I hate to admit it but Laird Renfrew may be right about the Hittite realm being the Indo-Hittite homeland. Still it ain't necessarily so, as the song goes; if Hittite et al represent a movement from the Balkans into Anatolia around 2000 BC as some archeologists think, or thought, then Moiety B becomes favored.

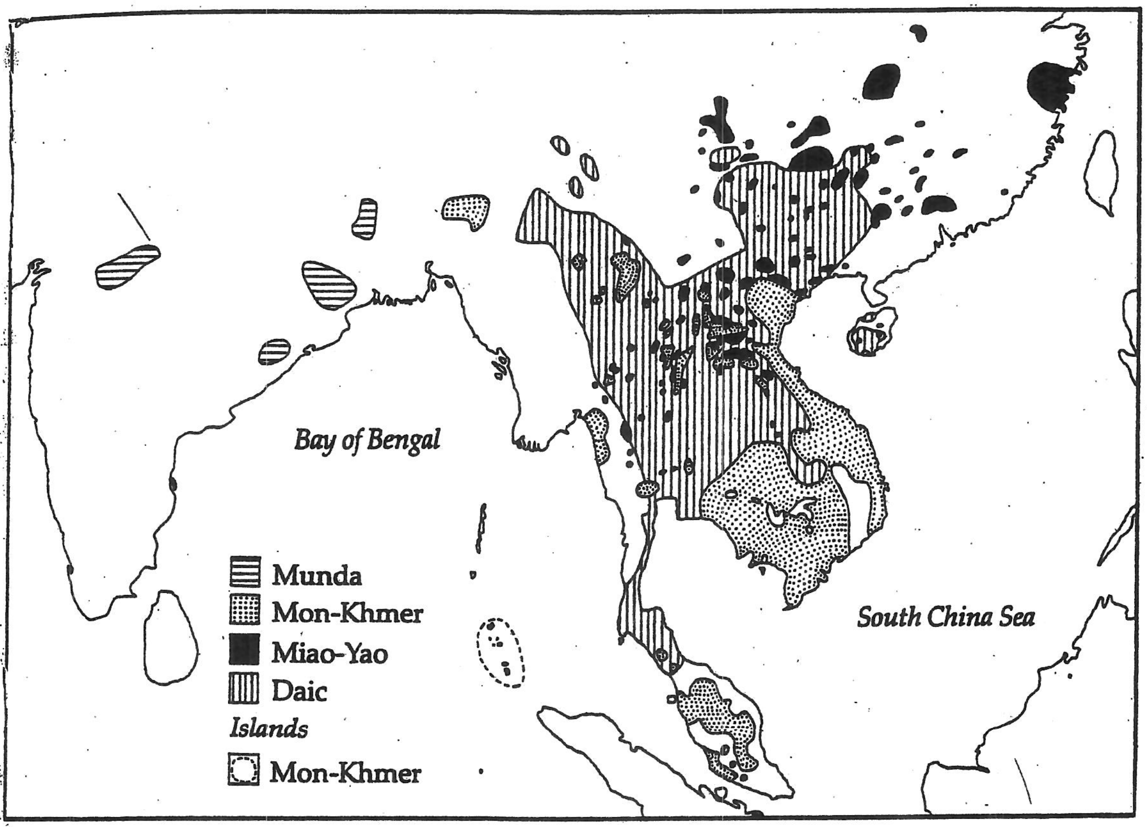
Or as Greenberg proposed before he died, if **Etruscan** joins Indo-Hittite as an external coordinate (a co-equal branch of **Eurasiatic**), then Etruscan becomes Moiety A and Indo-Hittite becomes Moiety B. Being known for sure only from north Italy, but with vague traditions of former residence farther east, Etruscan is roughly equidistant from Anatolia and southern Russia; its common period with proto-Indo-Hittite is necessarily **older** than proto-Indo-Hittite. Let us arbitrarily **reject Italy** as the common homeland of Etruscan-Indo-Hittite, giving those vague traditions more weight than they probably deserve. We are left with either Anatolia or southern Russia as our logical choice for the common homeland, although the Balkans look more attractive for non-linguistic reasons –that is where the **Neolithic** is settling in about that time and the Balkans are the focus of the Etrusco-south Russia-Anatolia triangle

Bringing in Etruscan illustrates our **second** choice of clues. Sometimes, like when we hit an impasse such as Moiety A versus Moiety B, we can bring in an **external relative**, someone related but not in the immediate family. Etruscan does that for Indo-Hittite. But consider what we can do when we are stuck with the Balkan triangle. We can bring in another outside moiety, e.g., **Uralic**. Since there are now three of them, we cannot call them moieties any longer. Anyway without getting into definitional problems one thing becomes very clear – the whole scene is pulled strongly to the **east and north,** towards the Volga river system or the Ural mountains, and the date is even earlier. Thus in our Sino-Tibetan example Moiety A (**Bai**) is struggling with Moiety B (**Chinese**) until we bring in Tibetic, Burmic and Karen which pull the whole scene west and south, effects similar to those of Etruscan and Uralic on Indo-Hittite. Now supposing that our calculations have put Sino-Tibetan somewhere more southerly and western like Yunnan or Sichuan. Need we stop there or settle for this as a homeland?

Well, we can confront Sino-Tibetan with two older external relatives, those proposed by Nicholaev and Starostin. First, **Yeniseian** is today found straight north from Tibet and about 1500 miles away. A reasonable presumption is that **Ket, Kot, et al** moved some distance north to escape the horse herders of Kazakhstan or Mongolia and could have an older homeland closer to the Altai or even Xinjiang itself. Before deciding whether Yeneseian moved or Sinto-Tibetan moved, we can bring in another relative to help out, namely **Caucasic (North Caucasic)**. This is exceptionally clear from the geography. It would be far easier for their connection to Sino-Tibetan and Yeneseian to be via the **great grasslands** which lie just to their north and extend all the way to the Altai than to be connected via the **high mountains** and deserts to their south to northeast India. Assuming that this analysis is correct, the impact of Caucasic is to pull Sino-Tibetan to the north –I would bet on Xinjiang or north Tibet as the Sino-Tibetan homeland.

The **third or last** set of clues is one whose relevance and persuasiveness may be immediately apparent. In any region for which a homeland is proposed one must check to see if **some other** linguistic group, preferably unrelated or not closely related, has a clear claim to **priority** in it. For example, in the case of Anatolia the Indo-Hittite homeland was always troubled by the presence of **Hatti, right under** the Hittites, plus the general likelihood that two branches of Caucasic (West and East) had a documented presence from the northwest of Turkey to **Hurrian** on the east. There were also West Caucasic loan words in ancient Greek. In Armenia, Hurrian clearly was there before Armenian.

In the case of our Sino-Tibetan problem we note that Chu et al list a number of non-Sino-Tibetan languages in parts of south China. The question then becomes: were any other language groups present in strength in various parts of China who might challenge the particular region as a Sino-Tibetan homeland. In the cases of north China and most of Tibet there is no evidence of other language groups. In south China, Taiwan, and southeastern Tibet there are other groups in strength. See Map 2 for the mapped distributions of four families with possible claims on the areas in question. Taiwan is not shown on the map but its Chinese majority sits on an island with several sub-phyla of Austronesian still in residence (e.g., Atayalic, Paiwanic, Tsouic). Moreover, according to documented history, Fukien Chinese began settling Taiwan in the early 7th century AD.



Map 2. The Miao-Yao, Austroasiatic, and Daic families

Of the four families shown on Map 2 one, **Munda,** is restricted to India and thus out of play for China. But **Mon-Khmer** seems to take precedence in mainland Southeast Asia, being represented in the far west (Khasi), the far south (**Aslian**) and **Nicobar Islands**, the far east (**Vietnam**) and north into Yunnan (**Blang** of Chu et al). Mon-Khmer is not competitive in China, however, and indeed shares a major portion of Southeast Asia with Daic. In majority opinion, as far as we can tell, Munda and Mon-Khmer join together to form **Austro-Asiatic** which has to be given priority in eastern India and Southeast Asia south of China.

In China proper, south of the Yangtze river, there appear to be two dominant families, Daic and Miao-Yao. Both are represented several times in Chu et al's sample.

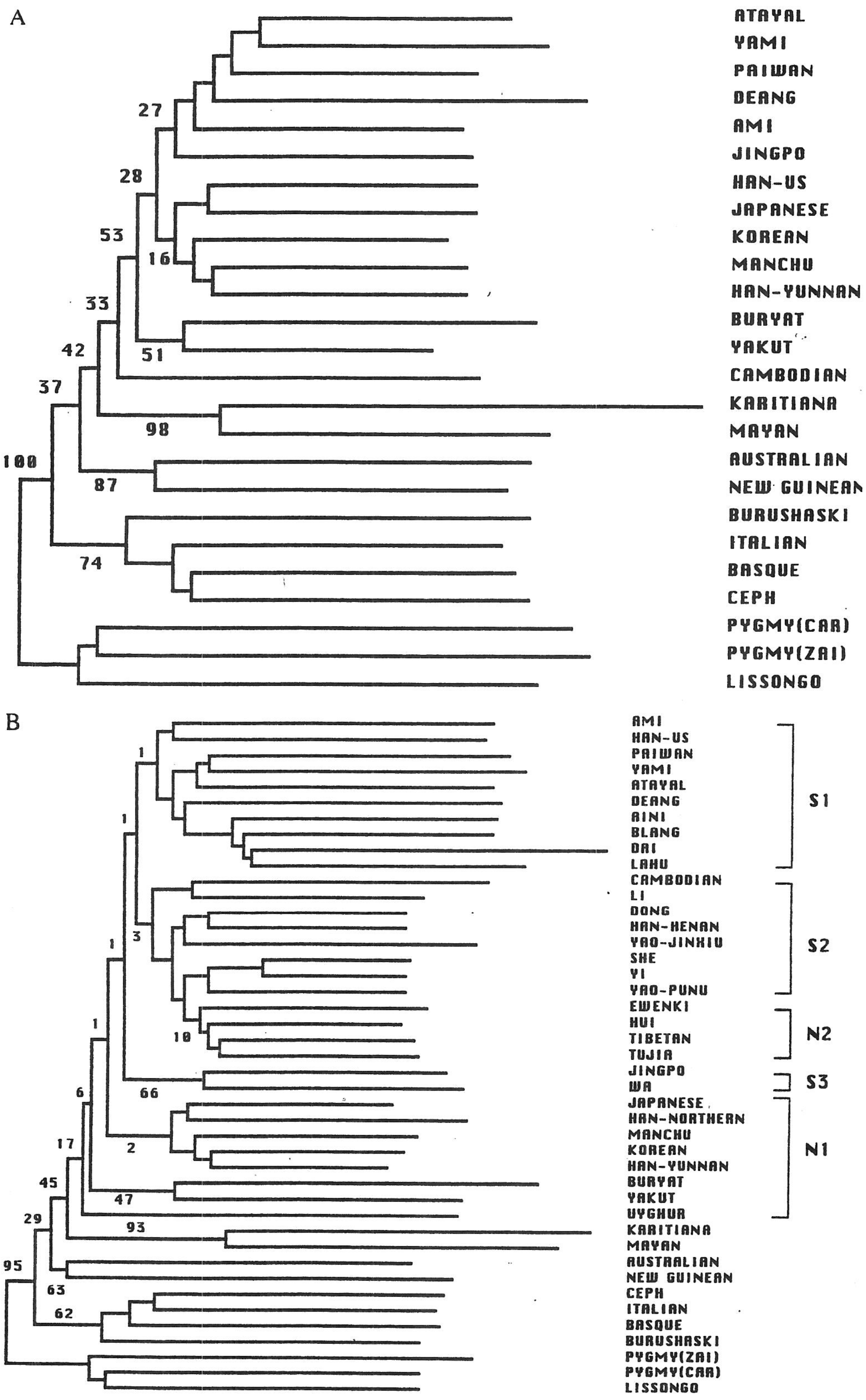
However, the two are not closely related and not related at all in many opinions, so they must be treated separately. Daic is said by tradition to have done two things, viz., they either started the south China Neolithic or at least passed the crops on to north China and they moved south into mainland Southeast Asia because of pressure from the Mongol conquest of China. And because Daic is most frequently related to Austronesian, and usually called Austro-Thai, it seems fair to include the eastern provinces from Shanghai to Hainan in their range. Their association with the south China Neolithic further suggests that they have at least 10,000 years time depth in southeast China.

Miao-Yao is the usual Anglophone term for the widely scattered set of languages which are known to Chu et al as **Hmong-Mien**, divided into **Hmongic** and **Ho Nte.** There are settlements of Hmong in the United States, refugees apparently from the Vietnam war in Laos (probably). The usual testament to a language's remoteness from any kinfolk is difficulty in classification. Like Sumerian and Basque, Miao-Yao has been quite difficult. If we follow Paul Benedict and Joe Greenberg (in 1980 as reported in Ruhlen's GUIDE), Miao-Yao is a prime sub-division of **Austric,** a kind of Nostratic for southeast Asia. Its fellow primes in Austric are Austroasiatic and Austro-Thai, both large phyla with many members and considerable time depth. So, despite their ostensible traditions of having come from the north, Miao-Yao are the odds-on favorite for the role of **south China's autochthone** prior to the Neolithic.

Further Evaluation of Chu et al's Taxonomy and Prehistory

From the basically linguistic analysis up to now we can say that we agree with Chu et al's first conclusion about China – that the northern and southern populations are distinct. It is not clear why they think that the northern populations are **biphyletic** with the implication that the southern populations are not. A brief look at Figure 1** (over leaf) will show how arbitrary that assessment is. There seem to be a lot more northern populations than their analysis allows for—the reason being the inadequacy of their sampling. Such peoples as the Mongols of the lower Gobi (in Chinese territory), the Bai, and the many ethnicities related to Tibetan were not sampled. In traditional/classical genetic studies, including Gamma Globulin studies, the Tibetans, Mongols, north Chinese, Koreans and Japanese formed a class by themselves. No one knows what genetic conclusions have been reached about the Bai because no one (that I know) has studied them. Even an old fashioned anthropometrical study is not known (to me). Just knowing their average height would be useful, since that has some evidential value in eastern Asia.

Their comment about linguistic boundaries being transgressed because of gene flow between populations is a point very well taken. China has some very famous cases where linguistic borrowing, presumably accompanied by gene flow, has so complicated the taxonomic analysis that erroneous results have occurred. The most famous case, of course, is that of Thai/Siamese vis-à-vis Chinese where their fairly close relationship was standard classification in historical linguistic textbooks for many years, until Paul Benedict challenged that conclusion by showing the huge number of loan words from Chinese in Thai and the substantial number of old Thai words in Chinese. The same sort of thing has affected Bai where borrowings from Chinese have obscured the true relationship. Bai was normally treated as 'just another Chinese dialect', if not ignored .


A
ATAYAL
YAMI
PAIWAN
DEANG
AMI
JINGPO
HAN-US
JAPANESE
KOREAN
MANCHU
HAN-YUNNAN
BURYAT
YAKUT
CAMBODIAN
KARITIANA
MAYAN
AUSTRALIAN
NEW GUINEAN
BURUSHASKI
ITALIAN
BASQUE
CEPH
PYGMY(CAR)
PYGMY(ZAI)
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ATAYAL
DEANG
AINI
BLANG
DAI
LAHU
CAMBODIAN
LI
DONG
HAN-HENAN
YAO-JINXIU
SHE
YI
YAO-PUNU
EWENKI
HUI
TIBETAN
TUJIA
JINGPO
WA
JAPANESE
HAN-NORTHERN
MANCHU
KOREAN
HAN-YUNNAN
BURYAT
YAKUT
UYGHUR
KARITIANA
MAYAN
AUSTRALIAN
NEW GUINEAN
CEPH
ITALIAN
BASQUE
BURUSHASKI
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PYGMY(CAR)
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1
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3
1
1
10
6
66
17
2
45
47
29
93
95
63
62

FIG. 1. Phylogenies constructed by using the neighbor-joining method based on 30 microsatellites (*A*) and 15 microsatellites (*B*), respectively (12–14). Numbers on the branches are bootstrap values based on 500 replications. See text for discussion of clusters S1, etc. indicated on the right.

When Chu et al submit that *Homo sapiens* does not have an independent origin in China, we can only agree. Let our colleagues in genetics and paleoanthropology decide the matter; it is fundamentally a physical subject.

Finally, Chu et al's statement that "The phylogeny also suggested that it is more likely that ancestors of the populations currently residing in East Asia **entered from** Southeast Asia" is **largely true** in terms of the genetic and linguistic evidence they and we have presented. However, it is not totally true. The fly in the ointment is the northern tier of Uighur, Mongol, Manchu, Korean, Japanese and the northern Han Chinese. Except for the Chinese, all of them can be **derived** genetically and linguistically from the **north.** This is perhaps most telling in the case of the **Uighur** (their Uyghur) who are not only **Turkic** in speech, inheritors or descendants of Buddhist Indo-Europeans (Tocharians), and marked by 25% of their genome acquired by millennia of intermarriage with European type people. Were one to try to derive the Mongols and Manchu from Southeast Asia one would at least run the risk of being laughed at; their situation is so obvious. Korean is largely the same. But Japanese is controversial. There is a strong tradition of southern origin of many elements of Japanese culture and their linguistic classification is not close to being settled; they are either (a) unrelated to anyone else, like Indo-European, or (b) they speak an Austro-Thai language, or (c) their language is related to Korean and beyond that, Altaic. Moreover, the **whole lot** of these northerners belong to Greenberg's **Eurasiatic** or Bomhard's **Nostratic,** except for Chinese and Sino-Tibetan which belong to Fleming's **Borean** along with Eurasiatic and Dene-Caucasic.

The **logic** of the geneticist's position that East Asia was populated from Southeast Asia is **troublesome**. Basically, it assumes that the family tree will reveal **directionality** of movement, as well as degrees of relationship, **near-ness** and **far-ness**. If non-Africans are farther from Africans than they are from each other, then the presumption is that the non-Africans moved out of Africa to reach their present locations. But as we saw with the Indo-Hittite discussion, it ain't necessarily so! But that presumption is quite reasonable! Underlying this presumption probably is **William of Occam's** famous razor. One prefers the most economical/parsimonious assumption over a complicated one. Why assume the rest of the world is the homeland from which Africans moved to Africa? It is simplest or most economical to assume that the rest of the world was settled in one exodus from Africa. Or even two or three.

The **answer** to William of Occam and our presumption is that problems sometimes have **parts** to them. Many parts may be subject to one explanation but some parts may differ, not be subject to that explanation. And the exceptional part in Chu et al's perfectly reasonable conclusion is Sino-Tibetan; it did not follow the lead of Munda, Mon-Khmer, Daic, and Miao-Yao. Rather it went the **other way**. As we have tried to show linguistically, Sino-Tibetan like Han Chinese itself is a north to south phenomenon. Genetically, as shown by Chu et al's Figure 1 (part B), Han-Northern is a fellow of a group which includes Japanese, Korean , Manchu, Buriat, Yakut and Uighur –so-called 'N1' – along with unexpected Han-Yunnan. The **'Sino-Tibetan had a northern origin'** hypothesis rests its case.

* Permission granted to reproduce Maps 1 and 2 from pages 142 and 149 of Merritt Ruhlen's GUIDE TO THE World's Languages, Vol 1, Classification, Printing with Postscript, 1991. Stanford University Press, Stanford.
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PREHISTORY IN *ITS SOCIAL & SEMANTIC ASPECTS*

Currently, the social and political world of most of North America is absorbed in a debate which first occurred when Charles Darwin and his friend Thomas Huxley challenged the veracity of the Christian theory of **creation** with the scientific theory of **evolution** in mid-19th century Britain. Although the debate smoldered quietly through the rest of the 19th century, it burst into flame again eighty years ago in Tennessee, during the famous Scopes trial. Thereafter the debate's embers stayed warm in the American South and among evangelical Christians for the rest of the 20th century. Now it has burst into flame again, this time with so-called creationist Christians including the President of the United States and with a majority of Americans deciding that "it would only be fair" if both theories were taught in public schools below the college level. Many scientists are outraged by this battle, insisting with anger and considerable arrogance in some cases that creationism is not in any serious way a scientific theory.

The reason for mentioning this matter at all is because Merritt Ruhlen sent me a copy of some things said at Joseph Greenberg's memorial service at Stanford in 2001. Among them was a Biblical passage which reminded everyone that historical linguistics was involved in its own way with a strikingly parallel discussion with a Judeo-Christian theory of creation in a different sphere. Although there is no hot debate, nor any hard feelings, the Judeo-Christian theory is interesting..In Genesis 11.1-11.9 we read:

> Now the whole earth had one language and the same words
> And as they migrated from the east, they came
> upon a plain in the land of Shinar and settled there.
> And they said to one another, "Come, let us make
> bricks, and burn them thoroughly" And they .had
> bricks for stone, and bitumen for mortar.
> Then they said, "Come, let us build ourselves a
> city, and a tower with its top in the heavens, and let
> us make a name for ourselves, otherwise we shall be
> scattered abroad upon the face of the whole earth."
> The LORD came down to see the city and the
> tower, which mortals had built.
> And the LORD said, "Look, they are one people,
> And they have all one language, and this is only the
> beginning of what they will do; nothing that they
> propose to do will be impossible for them.
> Come, let us go down, and confuse their language
> There, so that they will not understand one
> another's speech."
> So the LORD scattered them abroad from there
> over the face of all the earth, and they left off
> building the city.
> Therefore it was called Babel, because there the
> LORD confused the language of all the earth, and
> from there the LORD scattered them abroad over
> the face of all the earth.

A Trombetti Documentary

Jonathan Morris

Introduction

In the Summer of 2003, I discovered copies of two books by Alfred Trombetti in the London Library: the extended essay, L'Unità d'Origine del Linguaggio (1905) [The Original Unity of Language or Ud'O], and his final 750-page magnum opus, Glottologia (1923) [Glottology or G], both unread since they were donated to the same institution in 1927. At this point, I knew precisely two facts about the man: that he had proposed a single origin for human language and that his academic reputation had been destroyed as a result. In fact, this is about as much as one gleans from the secondary literature, and I have only found two authors who cite him: Merritt Ruhlen who gives a few approving but cursory lines on his early work, Ud'O, and Larry Trask, who was unable to muster his usual zeal to shoot Trombetti's Basque-Georgian cognates down in flames. Trask's lead to Greenberg as a 'modern sympathiser' also turned out to be a disappointment, since Merritt Ruhlen assures me that Greenberg never actually read him (an interesting point to which I shall return). Trask also repeats the Greenberg's remark that Trombetti's enthusiastic embrace of mass comparisons led to his being "practically run out of the linguistics community"[1], but this too appears to be wide of the mark.

Having read him, this obscurity is little short of astonishing, persuading me that even a modest general account of his work was required. Fortunately, this is a relatively easy task, since Trombetti writes such lucid Italian that his meaning is almost always clear. As far as possible, therefore, I have tried simply to 'point a camera' at him and let him talk straight to the reader. Hence the title of this article.

Unfortunately, while G and Ud'O are probably the most important and synthetic of his works, they are not the whole of Trombetti and the remainder is hard to locate. They nevertheless mark the beginning and (almost) the end point of his mature career, and I have thus tried to use them to show how his views changed.

I nevertheless did succeed in locating the 1938 tribute volume of approving articles by well-regarded figures in linguistics such as Pisani, Migliorini, Tagliavini, Pallottino, Cuny and Trubetzkoy. These figures knew that Trombetti's knowledge of languages was probably unparalleled, but that in the words of his mentor, Hugo Schuchardt, "his work was a beginning, not an end".

In the light of this 1938 volume, Trombetti's admission to the Accademia Italiana and the printing of Glottologia at the expense of the local savings bank, it seems hard to believe Greenberg's remark that Trombetti became an outcast, making the subsequent obscurity of his work all the more baffling.

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[1] L. Trask, Historical Linguistics, p. 385.

Biographical sketch

The circumstances of Trombetti's childhood were hardly auspicious:

"I was born in Bologna in 1866...my parents were extremely poor, my family was numerous and the misery very great. I went to primary school, but don't remember whether I went to second or third grade..."
"evidently, my parents couldn't give me money to buy books, but if by chance, I managed to put together five or six coins, I would run to the stands of the second hand book sellers and buy a grammar or a cheap book to read".[2]

Trombetti found a French grammar and surprised his teacher with questions about how to pronounce 'u' or 'eu', sounds absent from Bolognese dialect. He found a German grammar, learned all the roots and could read Lessing's fables within two months. He studied an elementary Greek grammar, found a Hebrew grammar, albeit in Latin, and pestered his mother to take him to the local priest for Latin lessons. He then persuaded a Persian convert to Christianity to teach him Arabic, Persian and Turkish. Finally, a bookseller that he frequented took an interest in him and went to visit him:

"My mother (who didn't even know how to read) didn't know what to tell the bookseller, except that I studied day and night, but from the kinds of books that were shown to him, he immediately understood the situation. He soon spoke of me to his many customers, who like him considered my case to be interesting and decided to invite me to a meeting to show what I knew. The bookseller spoke to me of this and I told him I was ready to meet anyone."

At the age of 18, Trombetti thus found himself before a panel of distinguished academics, including Giosuè Carducci and the noted Latinist, Gandino. Having acquitted himself on the classics, he then impressed them with his knowledge of English, German, French, Spanish, Portuguese, Hebrew and Arabic and was rewarded with a pension from the local authority and a religious charity that allowed him to enrol at the university in 1884 and graduate in 1891.

Despite this promising start, Trombetti spent the 1890s moving around Italy as an obscure secondary school teacher struggling to feed a large young family, and only publishing one piece in 1897 on relationships between IE and Hamito-Semitic, which he subsequently repudiated. It is not clear how he managed to carry on with his researches, but his major ideas on monogenesis seem to have occurred to him around the turn of the century, when he was living in Cuneo. His fortunes finally changed dramatically in 1904, when he was awarded the Royal Prize of the Accademia dei Lincei for an essay on relationships between Caucasian and Hamito-Semitic, by a panel that included the noted Austrian long-range linguist, Hugo Schuchardt and shortly afterwards, a lectureship in Semitic Studies at the University of Bologna, which was followed by a chair in 'Science of Language' in 1912. He appears to have enjoyed considerable academic success thereafter, with Mussolini personally giving him a grant to study Etruscan in 1928 and his appointment to the Accademia Italiana in March 1929, three months before his death.

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[2] Taken from the autobiographical sketch he wrote for the Royal Prize of the Accademia dei Lincei of 1904.

During this period, Trombetti published a steady flow of works, beginning with his first definitive statement of monogenesis, Unità dell'Origine del Linguaggio (1905), an essay, 'Come si fa la critica di un libro' [How to criticise a book] (1907), in which he responded to the critics of his monogenetic ideas, a preface to the comparative studies on Papuan-Australian-Andamanese by his student, Gatti (1906-09), the first study linking Sandawe to Hottentot (1910), a work on Elamite (1913), more extensive studies on pronouns (1908) and numerals (1913) and his Comparazioni Lessicali (1920), a compilation of 450 global roots, all of which were summarised in his magnum opus, Glottologia (1923), which also included much new material on the Americas. During the 1920s, Trombetti also published on Papuan-African cognates (1921), Basque and its relationship to Georgian (1925), Tasmanian-African cognates (1926) and spent his last years working on Etruscan. In 1929, he died of a heart attack aged 63, while swimming in the sea off the Venice Lido.

Monogenesis

"Initially with a practical and mildly philological intent, I set to the study of the main European and Oriental languages, until I read Karl Brugmann's 'Grundriss' in my twenties and was pushed for good into the field of comparative studies. Discovering the 'Grundriss' after Bopp's 'Grammar' and Schleicher's 'Compendium', I could hardly avoid being disorientated by the novelty of the doctrines expounded, whence I turned to comparative studies of Semitic, Uralo-Altaic and Dravidian. Returning to Indo-European after acquiring clear notions of manifold linguistic processes, it seemed to me that the Indo-Europeanists were dominated by rather limited ideas, and the unlimited faith I had had until then in the methods and results of Indo-European glottology was shaken. My reaction that followed led me, as was usually the case, to the opposite extreme." (G, Preface P. 1)

"At the start of the introduction [to UdO] it is stated that the intention of my studies was not originally to demonstrate the unity of human language but to establish definitively whether a genealogical link could be found between the Semitic and Indo-European languages, no matter how remote. It is worth repeating this for those who insist on seeing nothing more in my work than the monogenesis of language, and even worse, imagine that I was moved by a preconceived thesis." (G, Preface P. 2)"

"I was led to confront the problem of linguistic relationships in all its breadth for the reasons given in the introduction.[...] and having broadened the field, in 1902 stumbled unexpectedly on a series of precise correspondences between African numerals and those in the Munda-Khmer languages of India and Indochina, a fact of capital importance that many continue to ignore and that can only be explained by accepting a common origin."

"It was almost natural, however, and inevitable that my doctrine that was so decidedly monogeneticist should provoke incredulity and diffidence. Until then glottology had been dominated by the most absurd and unfounded polygenetic systems, supported by famous German masters" (G, Preface II).

"The question of the unity or plurality of the origin of languages passed through three stages or periods.

Initially, the unity was generally accepted, either on account of religious tradition or due to vague intuition, or due to insufficient if not false proofs. This was a period of pre-scientific dogmatism, in which the single origin of man was also admitted.
In the second half of the last century, Pott, Schleicher and F. Muller introduced the opposite dogma to science of the polygenesis of language. Given the great authority of these masters of glottology, it is hardly surprising that their theses, although unproven and unprovable, were followed by the majority without examination. In this way, honest attempts to connect one primary group to another were judged anti-scientific and condemned a priori with many withdrawing from fertile researches to the great detriment of the science. It is true that there was no shortage of authoritative voices (Max Müller, Whitney, Georg von der Gabelentz and others) who warned that they could demonstrate the relationship of languages rather than the contrary and that the possibility of a common language of all the languages of the world could be demonstrated. These voices, however, were too often overwhelmed by the cries of their adversaries, who set themselves up as unappealable judges and prophets and condemned in advance any one who wished to cast a glance beyond pre-established frontiers. At the same time, the unitary hypotheses was nevertheless recommendable, even as a simple 'working hypothesis', as Latham had recognized since 1849 "the more the general unity of human language is admitted, the clearer will be the way for those who work at the details of the different affiliations" (Opuscula 151)." [G pp. 189-90].
"The only genuinely scientific classification of languages, founded on a principle that may be applied consistently and without limit, regardless of any extrinsic criterion, is genealogical classification, which has always been a fertile source of important results, not only for the internal history of a language, in its nature, origin and evolution, but also for its external history, in the stories of peoples, and for many disciplines with close relationships to glottology [G. p. 10].
"The criterion of genealogical classification is given by linguistic affinity. It is nevertheless important to note immediately that this concept has nothing to do with that of resemblance: two related languages may be extremely similar, such as Italian and Spanish, or extremely dissimilar, such as Italian and Armenian. Languages are related when they are different continuations of the same language" [G. p.11]
"Finally, our science has a moral undertaking. Due to its original unity, on the one hand, and its successive division on the other, language is the symbol of a sublime accord between humanity and nations, and the study of it by "revealing affinities between the most apparently diverse tribes assists the principles of tolerance and fraternity between nations" [G. pp.5 – 6, part in inverted commas is attributed to Graziadio Ascoli].

A number of points emerge from the above quotations:
Firstly, Trombetti had a very well-articulated notion of where he stood in a historical process, having far greater sympathy for the data-based analysts of the early nineteenth century, than the German theorists of the later part, despite the methodological limitations of the former and methodological progress of the latter. He singled out two linguists for particular criticism, Müller and Finck.

Müller had based his polygenetic classification on the ideas of the German biologist, Ernst Haeckel, who proposed around 1860 that there had been various races of primitive men in remote prehistory, but that only two had survived, the straight-haired and greasy-haired, which had given rise to 12 extant races. Müller maintained in various publications appearing between 1868-76 that language had emerged after the differentiation of mankind into races, identifying 78 completely independent linguistic groups and claiming that a single race could give rise to several independent languages. As Schuchardt pointed out before Trombetti, the claims that Basque, Indo-European, Caucasian and Hamito-Semitic had all arisen independently of each other, albeit from the same Mediterranean racial stratum, strained all credibility, and the linguistic evidence (e.g. the links between Polynesian and Melanesian speakers) quickly showed that languages were no respecters of racial boundaries. Trombetti further rejected (and I shall return to this point below) this idea of racial differentiation preceding the emergence of language for the simple reason that he felt that the differentiation had to be accompanied by a geographical dispersion from an original home, which would have been impossible without the social organisation that language conferred.

It was nevertheless one thing to reject a poorly constructed polygenetic theory and quite another to reject the notion of polygenesis per se. In my view, Trombetti's motivations are apparent from the following quotation:

"The fundamental problem that Franz Bopp set himself, that of the origin of grammatical categories, could not be solved with the data provided solely by Indo-European languages. It was necessary to extend greatly the comparisons and enquire into the processes of the more archaic languages themselves." [G. p.3]

Hence, Trombetti had seen a small number of grammatical elements (mainly prefixes and suffixes, as will be seen below) repeated across all the world's languages, and if polygenesis were true, he would have to deny *a priori* any relationship between them. In other words, his data forced him into a monogeneticist stance.

Returning to Finck, the German linguist who corresponded with Trombetti considered that the supreme purpose of glottology was to discover the psychological basis for the differences between languages, and in his 1901 account, attempted to classify languages on a two-fold basis: degree of excitability (low, medium and high) and whether they tended towards the expression of 'Empfindung' presumably what we would term 'sense impressions' and 'Gefühl', or internal feeling. By this token, African languages were highly excitable 'Empfindung' type languages, while Polynesian, Papuan and Australian languages were excitable 'Gefühl' type languages, with (not surprisingly) balance represented by European languages. As Trombetti pointed out *"No one had ever shown less ability than Finck to feel the spirit and fresh vitality of primitive languages"*.

While Trombetti appreciated that Finck was not inherently hostile to the idea of monogenesis, his response to the latter's theory sheds significant light on his own 'data-driven' stance that led him to suspect grand schemes based on preconceived ideas about the nature of race and the relationship of language to thought. This by no means implied, however, that Trombetti ignored the latter:

"But the story of human language faithfully reflects the history of man, in both his internal and external development. This double function had already been inferred by Leibniz, when

he wrote that languages are the best mirror of the human spirit and that nothing shed more light on the investigation of the ancient origins of peoples than comparing languages. Our concern is to reconstruct in broad terms the most ancient history of mankind [...] Now this has been made possible precisely on account of the enormous fragmentation and differentiation undergone by language over an enormous number of centuries and in every corner of the habitable earth; ...such an advantage would be denied had language remained fixed and unchanged, or been subject to few divergences that would not have made it possible to go back to the epicentre." [G. p. 5]

"Thus I cannot agree with Schuchardt that the entire purpose of glottology consists only in forming a clear idea of the origin of language.....Glottology is the story of human language in its totality, from its origins to the present day..." [G. p. 5]

More importantly, however, it illuminates another avenue of attack on the polygeneticists who explained the growing number of cognates found between languages with an appeal to the "allgemein menschlich", [general human], what we might term a linguistic 'collective unconscious'. This view postulated the multiple emergence of language at different points in space and time, albeit with each such event reflecting the spontaneous execution of a common language blueprint. Trombetti argued that this position was self-contradictory, since if a single language blueprint existed then at least word roots should show some degree of similarity, but polygeneticists denied this on the basis of the diversity of language. It thus followed that they obliged themselves to argue for multiple blueprints, but the likely outcome of this would be very limited similarities, and certainly not the extensive ones that Trombetti observed.

Classification

Trombetti inherited two kinds of classification system: the morphological system dating back to Humboldt's classification (1822/1836) of languages into analytic and synthetic types (with the latter split into agglutinative, fusional, and polysynthetic), for which, unsurprisingly, he had little sympathy, on the grounds, firstly that modern languages could show hybrid behaviour (e.g. Finnish showing fusional and English analytical behaviour, despite being classified as agglutinative and fusional languages respectively), and secondly, that a genealogical approach illuminated the evolution of languages from one category to another, hence modern Chinese had not been analytic since the dawn of humanity but represented the end result of a process of simplification and tonalisation that a clearly related but agglutinative language such as Tibetan had avoided.

The second kind of system was the race-based system, discredited by the wealth of ethnographic evidence emerging in the last two decades of the nineteenth century, notably from Papua New Guinea and Africa. Trombetti appears to have begun with Müller's 1888 classification into 52 groups and simply worked away at 'joining the dots' until by 1905, he had arrived at 11 major groups:

Africa – 1. Bantu in the South, 2. Hamito-Semitic in the North
Eurasia – 3. Caucasian, 4. IE, 5. UA, 6. Dravidian, 7. Indo-Chinese, 8. Mon-Khmer
Oceania – 9. Malayo-Polynesian, 10. Andamanese-Papuan-Australian,
America – 11. American (at a very high level).

Trombetti notes that there was nothing original about his classification of groups 1 to 9, but that he had been the first to see the unity of 10 (and that Gatti, who published a study in 1905 showing the links between Papuan, Australian and Andamanese[3] had taken the idea from him), and that he had conceived a single Amerind family *"on the basis of few but sure elements (e.g. the first person pronoun, n- and the second person pronoun m- from the far North to the Far South)".*

He also noted connections between the families:

"The close connection of Mon-Khmer and Malayo-Polynesian was recognised by myself independently of W. Schmidt (1906). And I had already written on p. 5 of Ud'O that ***"to the languages [of SE Asia], particularly those of the Mon-Khmer group, we may link the Malayo-Polynesian languages,....while the remaining languages of Oceania, which we may group in an Andamanese-Papuan-Australian group, show more marked relationships with the Dravidian languages."*** *"* [G. p.18]

He also noted the higher-level relationship between these languages and the languages of Africa.

"Of the two oceanic groups, Dravidico-Australian thus coincides to a greater degree with Hamito-Semitic than with Bantu-Sudanese, although the branch of Oceanic negroes (Andamanese-Papua-Australian), on account of their archaic character, comes much closer to the African Negroes".
For the same reason, the other Oceanic group, Munda-Polynesian has more than a few elements in common with Hamito-Semitic, but most of these are also found in Dravidico-Australian. Hence ***ai*** *(I) is common to Nuba (With Songhai) and the Australian and Malayo-Polynesian languages, while Malayo-Polynesian* ***kamu, komu*** *(you pl.) is only found in Semitic* ***–kumū*** *(your). Here are some more pronominal forms common to the three groups:*
Hamito-Semitic Nuba ***ter*** *(these), Dravidian* ***tāru*** *(the same pl.), Munda-Polynesian* ***tar, ter*** *(these)*
H-S Kun. ***áme*** *(we exclusive), Drav* ***āme*** *(we)* ***ēm*** *(we exclusive), Munda-Poly* ***ami*** *(we)* ***yami*** *(we exclusive)*
H-S-Assyrian ***anāku*** *(I), Drav-Tamil* ***ennaku*** *(to me), Munda-Poly* ***inaku*** *(I)*

There can be no doubt that Munda-Polynesian is closer to Bantu-Sudanese than to Hamito-Semitic, even though it is further removed geographically from the former. The connection is shown to be extremely close above all in the numerals, then in the personal pronouns

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[3] Trombetti provides a list of 27 cognates between Semang and Andamanese/Australian and also points to Narrinyeri/Andamanese morphological similarities: cf. [You Sing]: Narr. ngurra, ngurre/A. ngolla, ngu-le; [He] Narr. ki-tje, ki-le/A. (Kede) ki-te, (Juwoi) ki-le; [Who?/What?] Narr. mei-ke, mey-a-k/A. me-če (who?), mi-a-k, me-a-k (what?); [Two] Narr. ninka-ienk, ninga-u/A. (Oenge) ninaga. He nevertheless refers to the work by his 'disciple' Gatti (1906-09), which established the lexical unity of Papuan/Australian/Andamanese to his own satisfaction. Trombetti also draws long-range parallels between Bantu and Andamanese prefixes, e.g. B. aka-mwa (mouth)/Bea aka-bang-da, Bale aka-boang etc. (mouth); B. ele- (one thing of two)/Bea/Bale i-dal, Kede er-tol (eye); Kede ir-pol (two). [G. p. 634/642]. T. evidently felt that there was nothing 'isolated' about the Andamanese languages.

(including pre-verbal forms) and in many other grammatical and lexical elements. Here are the forms of the preverbal pronouns as they appear in Pron. 199

	Melanesian	***S Bantu***
I	*i-, ya-* *ni-, na-*	*i-, ya-* *ni-, na-*
You (sing)	*u-, o-,*	*u-, o-*
He	*i-, e-, u-*	*i-, e-, u-*
We	*ti-, tu- (inclusive)* *ma- (exclusive)*	*ti-, tu- (inclusive)* *ma- (exclusive)*
You (pl.)	*mi-, mu-*	*mi-, mu-*

After Ud'O, his ideas continued to crystallise, the most notable changes being the grouping of Hottentot-Bushman and Sandawe in 1910 and its subsequent grouping as 'Southern Hamito-Semitic'[4], and the amalgamation of Dravidian with Papuan-Australian-Andamanese to form Dravidico-Australian.[5]

Also of interest is his comment on the Palaeosiberian languages:

"[these languages] were always considered by myself as intermediate between Uraloaltaic and American, firstly being closer to the latter and then decidedly associated with the former". [G. p. 18]

In this way, he arrived at a mature classification into two main branches: **Austral**, consisting of 1) Bantu-Sudanese, 2) Hamito-Semitic, 3) Dravidico-Australian and 4) Munda-Polynesian and **Boreal**, consisting of 5) Caucasian, 6) IE, 7) UA, 8) Indo-Chinese and 9) American. [G. p. 19].

As noted above, Trombetti had begun by hypothesising an intermediate position for Indo-European between Hamito-Semitic and UA, to which he also associated Dravidian. In this, he evidently anticipated Greenberg, abandoning his initial 'Nostratic' stance by expelling Hamito-Semitic and Dravidian and moving much closer to a 'Eurasiatic' one, in which this group was much closer to the modern Amerind and Dene-Caucasian.

Trombetti on Human Origins and Migrations

Clearly, just as the polygeneticist stance necessarily implied the defence of a kind of pre-Chomskian 'hard-wiring for language' theory, Trombetti's monogeneticist stance translated into an onus to explain the spread of humanity and differentiation of language, as well as to postulate an original homeland.

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[4] He also shifted the Nilo-Saharan languages from a branch of 'Bantu-Sudanese' to 'proto- Hamitic', closest to the Agaw languages, more or less adopting Reinisch's views in G. [pp. 38-40]

[5] On Dravidico-Australian, T. gives limited cognates in G. [p. 82] (e.g. Tamil engal- (we exclusive) = Austr ngali, ngule, ngadlu, etc. Drav nām/num = Narrinyeri nām/nōm [we, you], but there is a wealth of material in Gatti (1906), which I am in the process of transcribing.

In G. he rises to the occasion with a full chapter on prehistoric/anthropological correlates with his theory, although as should be clear, his theories were entirely rooted in linguistic data.

Even in the 1920s, knowledge of the fossil hominid record was still scanty. The scientific world was well aware of Homo Sapiens and the Neanderthals, but knowledge of earlier species was restricted to Homo Heidelbergensis and the notorious Piltdown Man (on which Trombetti had the good sense to avoid passing judgement). Trombetti praised Boule's 1921 treatise[6] on human palaeonotology, even though the duration of the latter's geological eras was about a thirtieth of our modern ones and had the dinosaurs disappearing only 3 million years ago. Trombetti nevertheless circumnavigated these limitations to arrive at a strikingly modern theory.

"*I imagine the origin and evolution of Man and Language in general as parallel processes. To the precursor of man corresponds pre-human, unarticulated language. The transformation of the precursor into 'Homo Sapiens' was slow and gradual like the transformation of pre-human language into true human language. Both happened only once, in a more or less extensive area of India, as I believe, and due to the convergence of a complex of favourable factors*" [G. 306]

An extremely important point is that between 1905 and 1923, his views on the antiquity of language changed radically:

1905:
"With regard to the antiquity of the human genus, this is certainly great in some parts of the globe but cannot be as enormous as some would have us believe. Since language is of the same age as man, who distinguishes himself from the beasts precisely on account of this, we can also establish a broad maximum and minimum. Indeed, the antiquity of language cannot exceed a certain maximum, otherwise linguistic groups would be more numerous and their divergence would be greater than it is, whence we would be unable to recognise the original unity; nor, on the other hand could it be less than a certain minimum, otherwise the linguistic groups would be less numerous and their divergence smaller than it is. Thus, taking account of linguistic differentiation, which on average takes place over a given time, I believe that I can state a minimum at 30,000 years and a maximum of 50,000 years, albeit with the understanding that these are figures given with the greatest of reservations." [Ud'O, p. 57]

1923:
"The dates of human palaeontology and geology do not permit, so it seems, the dating [of the emergence of language] to less than 100,000 years ago. For the development of language and the great successive differentiation it is necessary to accept an extremely long duration. In Ud'O, I gave (with many reservations) figures for the minimum and maximum that were too low: Today, I have no difficulty in establishing a minimum of 100,000 and a maximum of 200,000 years or more, if necessary. The reason is that through an enormous amount of evidence, I have now convinced myself of the great and marvellous

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[6] Marcellin Boule, Les hommes fossiles, Éléments de Paléontologie Humaine (Paris 1921)

stability of language. All of my work is a continuous demonstration of this truth, and it will however be opportune to give some particularly notable examples. I choose for this purpose some extremely old forms of the numerals 2 and 3.

Tasmanian and Papuan forms of the numeral 2 are found in the American languages:

Tasmania	***ka-la-bawa***	*Papua*	***la-bui***
Tasmania	***ka-la-bawa***	*Papua*	***ra-bui***
Terraba	***k-ra-bu***	*Gr. Pano*	***ra-bue***
Sabanero	***g-da-bu***	*Gr. Pano*	***da-bui***
Gr. Pano	***ka-ra-bö***	*Klamath*	***lá-pi***

We may add Somali ***la-ba, d□-ba****, from* ****da-u-ba*** *= Galla* ***lá-u-wa****, cf. Australian 137*[7] ***lo-ba*** *(pair, both), and we note that* ***–u-ba*** *coincides exactly with Sanskrit* ***u-bhá*** *(both). Note also Papua (Hagari and Iberi)* ***a-bui****, on the one hand with Bribri* ***bui*** *(two) and Lithuanian* ***a-bù****, Fem.* ***a-bì*** *(both). Finally Cushitic* ***lamma*** *(two) stands for* ***lamba****, Cf. on the one hand, Latin* ***ambo****, Mordvin* ***ombo*** *(other) = Tupi* ***ambo-aé****, and on the other Caripuna* ***erambué*** *(two)*

Now observe the following forms of the numeral 3:

Australia	***ma-n-gur***	***kura-m***	***mun-gura-ba***
	n-goro	***gula-m***	***kuli-pa***
Papua	***mo-n-gul***	***kiri-mi***	***n-garo-p***
Uraloaltaic	***na-gur***	***kōro-mi***	***gur-ba***
America	***n-goro***	***kro-mo***	***kura-pa***
	-kula	***kra-mia***	***kule-p***

It is easy to see that the last column of 3 corresponds to the first column of 2 (Australian ***kar-bu*** *(three) = Terraba* ***kra-bu*** *(two), Papua* ***γalu-b*** *(three) = Terrava* ***kru-bu*** *(two), etc., and indeed three derives from one plus two, cf. Oru-Lopikop (Papua)* ***kone-khala-vi*** *(three), Tauata* ***kone*** *(one) and* ***mon-*** *also expresses unity. In the extremely common Australian type* ***kul-bari*** *(three), the second term is African* ***bari*** *(two).*
Considering that the ancestors of the Australians and Papuans moved away from Asian soil in the extremely remote past, we can but feel a sense of extreme wonder at such complex forms transmitted faithfully from generation to generation over an enormous number of centuries." [G. p. 307]

But can we take this date of 100-200,000 years at face value, since if we apply the same scaling factor to this date as we do to Boule's, we arrive at 3-6 million years?

"Boule writes of Homo Heidelbergensis that he was "perhaps only a pre-human, a precursor. We haven't the right to affirm, even if it is possible, that he spoke an articulate language, that he knew how to kindle fires or cut stones, that he was already Bergson's Homo Faber". We thus have no reason to consider him". [G. p. 313]
"Neanderthal man is not a precursor but a true man. By his remains have been found pleasingly worked stones, coals and ashes of fires that he knew how to light and feed

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[7] The numbers apparently refer to E.M. Curr, The Australian Race, Vol. IV, pp. 16ff, 1887).

(although remains of coals and ashes have also been found in deposits of the first quaternary). But since men of the Neanderthal type became extinct without leaving descendants, we cannot know anything of their language, which like their race, belongs to a desiccated branch of the great trunk" [G. p. 313]
"Direct ancestors of Homo Sapiens coexisted [with Neanderthals] in Europe, from which we can deduce that Neanderthals cannot be the progenitor of modern man. The latter became extinct leaving no inheritance and disappeared suddenly due to migration or extinction in situ (according to some exterminated by superior arriving races)." [G. p. 295]

On the basis of the similarity between prehistoric and modern remains, Trombetti concluded that differentiation into races had occurred in situ, and that the less sophisticated primitive man would have been subject to far greater selective pressure, hence *"who would seriously believe, as F. Müller does, that man in an alalic state would have been capable of carrying out vast migrations, and even crossing oceans"*. [G. p. 308]

As mentioned above, he also saw linguistic and cognitive abilities maturing slowly in a limited spatial area over a long period, which he dated at 50,000 years [G. p. 307] and that this process preceded any migration.

Given Trombetti's rejection of the notion of language spread between Neanderthals and Homo Sapiens, he appears to have believed that the transition from pre-language to language was restricted to archaic Homo Sapiens living in India. This evidently did not exclude the possibility that Neanderthals could have inherited and developed the same pre-language independently, but the fact that they were not in India would seem to exclude them from participation in the development of the specifically human language that Trombetti placed at the base of modern languages.

But why India? Clearly, his monogenetic views implied a localisable homeland of limited extent occupied by archaic Homo Sapiens during his Unitary Period. He excluded Australia and the New World on the grounds that they have no higher primates and that access to the latter would have been sporadic due to glaciation. The Far East and Europe were also rejected due to the lack of Palaeolithic evidence in the case of the former, and the fact that the latter was a 'cul de sac'. This left Africa and India as fulfilling the two criteria of primates and Palaeolithic evidence, notably lithics.

In 1905 [Ud'O], Trombetti had made vague references to 'some region of Eurasia', showing that he had already excluded Africa, albeit without giving convincing reasons for doing so. In Numerali (1913) he pointed more explicitly to India as the only limited area in which three major linguistic groups (Munda-Polynesian, Dravidico-Australian and Indo-Chinese) were represented, not to mention Indo-European, although he regarded this latter group as a late arrival.

He also cites Matthew's[8] suggestion of a homeland for Homo Sapiens on the Central Asian plateau, moving it further south to the foothills of the Himalayas to coincide with the *"numerous remains of anthropomorphs that lived at the end of the Miocene and lower*

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[8] Matthew, Climate & Evolution, 1915.

Pliocene. In the midst of such a ferment of life, human remains cannot be lacking, although they have not yet been discovered." [G. p. 301], as well as the same author's demonstration that India had been the epicentre for the dispersal of mammals and primates in particular, concluding that *"the same factors that determined the dispersion of the primates must also have determined the dispersion of humans"*. [G. p. 301].

His model for this dispersion was an extrapolation of Johannes Schmidt's 'Wave Theory' (1872) originally formulated to explain the spread of Indo-European, further postulating that the spur to migration had probably been the pursuit of dwindling supplies of game, which in turn moved further away, leading to yet more migrations.

Trombetti's belief that migration and survival in general required highly developed cognitive abilities led him to reject the concept of 'primitive' humans, citing evidence that Bushmen[9], Pygmies and the Aborigines of Australia and Tierra del Fuego had highly evolved cultures that were in no way inferior to 'civilised' men, and sophisticated survival skills to deal with a hostile environment[10].

"In general, it may be said that the peripheral regions furthest from the centre of dispersion were only reached by the first waves of migration (with the possibility of reflux). In Africa, the first stratum was that of the Negroes, followed by the Southern Hamites (Bushmen, Hottentots, then the Sandawe, etc.) and also in Oceania, the Negroes preceded other tribes. We may deduce from this that the languages of the extreme regions are the most archaic, explaining the apparently strange fact that geographically remote languages often agree with each other more than neighbouring languages"[11] [G. p. 206].

The prime examples[12] of this were his Munda-Bantu numeral comparisons[13] that he claimed as the starting point for his monogenetic theory:

One

*B **mue (*muai), mo-, mo-si***	*M-K: **mue, muai, mo, mo-s***
*Somali **mi-d***	*Kolhari **mī-d**, Annamese **mô-t***
*B **bo-***	*Savara **a-boy**, Lakadong **bi***
*Coptic **wai, wei***	*Khasi **wei***

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[9] On G. p. 312, Trombetti cites Schwalbe's demonstration that the Pygmies were highly evolved, without details, and this at a time when many linguists such as Klaatsch and Schmidt believed that they were a subhuman 'missing link', and in the latter case, even 'pre-Neanderthal'.

[10] At the same time, Trombetti did not idealise 'primitive man', noting that he had probably been a cannibal and may even have preyed on the Neanderthals

[11] On G. p. 168-69, T. applies the principle of "furthest = oldest" to deduce that the languages of South America are probably much older than those of North America, adding that "from the preceding considerations, we deduce that the closest relatives of the North American and Palaeosiberian languages should be sought in the Uraloaltaic and Indochinese groups, which are still geographically closer, while for the South American languages, which separated in remote times, when the current linguistic groups were not yet fully distinct, comparisons could also extend to the Munda-Polynesian and Dravidico-Australian groups.

[12] T. further illustrates this point on G. p.207 by comparing the Udi/Eskimo case systems and the Georgian-Basque conjugations, noting that the conjugation of Dakota is even closer to Georgian than Basque.

[13] Trombetti extended these cognates, especially to the languages of Oceania

	Two	
*B **ari** [T. every region of Africa]*		*Khasi **ār**, Lemet **ar***
b-ari		*M-K **b-ar**, Kolhari **b-ari***
	Four	
*B **ana**, b, **ku-ana***		*M-K **pu-on**, **pu-an**, Nicobar **fu-an***
*Bari **un-gu-an**, cf. B **na-ne** [eight]*		****un-pu-an**, cf. Nicobar **on-fo-an** [eight]*
	Five	
*B **tano***		*Nicobar **tan-ein**, Danaw **thön**, etc.*
*B **šano**, **sano**, Somali/Galli **šan***		*Khasi **san**, Mon **–sun**, Amok **hsen***
	Ten	
*B **kumi**, sometimes **šumi***		*Nicobar **šom***

According to Trombetti, names of animals still reflected these Asia to Africa migrations, notably words for *elephant* and *monkey*: [G. p. 304].

Africa		**Asia**
	Elephant	
Mutsaya ***n-džako***, Nteghe ***n-tsayo***		Mongol ***dzaχan***, ***saγan***
Kanyika ***zof***, Kum ***n-šoan***,Afudu ***e-foan***		Manchu ***sufan***
Bayong ***n-tsenya***		Mon ***tšin***, ***tšing***
Ngoala ***e-so***, Balu ***n-son***		Mo-so ***tso***, ***tson***
Mbe ***e-šan***, Bilin ***dž□nā***		Indochinese ***a-šang***, ***sang***, ***tšang***
	Monkey	
Gbaya ***mbulu***		Brahui ***bolu***
Kredj ***buru***		Tibetan ***s-preu***
Hausa ***biri***		Gyarung ***se-pri***

He also mentions similar cognates in the *Comp. Less.* for lion, leopard, crocodile and snake.

Trombetti made the most important point that:
"the greatest marvel that I have proven in the course of my investigations refers to the degree of development that human language had attained in the unitary period prior to the first great emigrations" [G. p. 209]
"even in the remotest times, we find both a notable degree of culture and well-developed languages. Here and there, especially in peripheral regions, a physical and cultural decadence followed, while language could often remain almost unaltered." [G. p. 315]
"the numbering system in the unitary period was already fully developed" [G. p. 212].

As an important consequence of these observations, he postulated that a large cultural vocabulary could be traced to the end of the unitary period. Since at this point, the degree of differentiation of different language groups would still have been limited, it did not make sense to speak of loan-words. For reasons of space, I shall merely cite some examples:

*"The first step on the path to civilisation was made by man when he mastered fire, having found the way to kindle, extinguish and make use of it. Among the most widespread words meaning fire we shall note the following: South Bantu **–koni** [fire, firewood] – e.g. Isoama **o-kō** (fire), Galoa **o-honi**, **e-koni**, **e-honyi**; Nandi **kweni-** (firewood); Sanskrit **a-gni-**, Latin*

**i-gni-**, Slavic **o-gnĭ;** Australian **keni**, **koonni-a**, **kuni-ka**, 163 **n-goon** = Tasmanian **n-gune** (=South Bantu **n-gun**); Duke of York **un-gan**, cf. Lithuanian **un-gni-** (fire); Eskimo **i-gne-k**, **in-gne-k**, Gr. Athabaskan **kone**, Paiute **qunna-** (fire), Zimshian **kun** (firewood), Guarauna **i-kunu-h** (fire),; Samoyed **tu**, **tū**, Dravidian **tī**, Savara **to-**, Chiqito **tū-s** (fire) (originally smoke = Mabuiag **tu** (smoke); Mande group **te**, **ti-a**, Dravidian **tī** (fire) (originally 'firewood'), Avatime **ke-fú**, Logone **fū**, Muzuk and Mandara **á-fu**; Malayopolynesian **a-pu**, **a-pui** and **a-pi** (fire), Sulka **a púi** (fire); Japanese **fī**, Thai **fāi**, Yang **fī**; Timote (S America) **fú** Paniquita **i-pi**, etc., Turkana **a-puru** (smoke), Greek **púr**, Samoyed/Sotyak **pur-ga** (smoke), Australian **puri** (fire, smoke), Mafoor **fōr** (fire) "[14]

"To the last series we may connect **pek-**, **pok-** (cook). Then we also have a very widespread noun for a cooking receptable Pul **hor-de**, Pl. **kore** (calabash) = Mbugu **χore** (pot), Galla **o-kole** (milk pot); Irish **ko^{i}re** (cauldron), Cheremis **kor-čak** (pot), etc.; Fiji **kuro** (cooking pot), elsewhere **kura**, **kure**; Choco (S America) **kuru** (pot). To Turkish **kazan** (cauldron) corresponds Chuvash **χuran** and Tungus **kalan**, with forms corresponding to these in the languages of Indonesia: **kuran**, **kalan** (pot)."

Many words meaning 'boat, ship' are very widespread. Nuba **kub** (ship), Pl. **kubl-ī**, Egyptian **kbn-t**, Andamanese Boj **kóbr-da** (canoe), Australian 24 **kibrea**, Bongu **γobu-n**, Bogadjim **χubu-n** (boat, ship).

Heb **oni**, **onijja** (ship), Arabic **iná**, Pl. **aniha-**, Doric Gk **nā-s**, Welsh **noe** (flat vessel), OE **na-ka** (NHG **Nachen**), Samoyed Jur. **ano**, **'ano**, Kam. **äńi**, Taihi **'an-du-i** (ship); Prob Yehen (New Caledonia) **won**, already compared with Jagan **ane-n** (boat, canoe), Alakaluf **a'un**, Ona **yeni**, Patagonian **yini**."

"even the idea of 'combing, comb' finds correspondences between extremely remote languages. In West Africa, Efik has **sat** (to comb), **edi-sat** (comb), and in Khasi we find **snād**, for **n-sād** (comb); cf. Tibetan **šad-** (to comb, to brush)."

"Words with an abstract meaning are also widespread.....:Bantu **gan-** (think, know) (whence **–gan-dsa** (doctor), Pul **gan-da-l** (knowledge), Somali **k□□ān** know, be familiar with (=Isubu **kane**, be acquainted with k < g), Geogian **gon-** (think), IE **gjen-** (know), Manchu **gōni-** (think), Vogul **χań-ś**, Dravidian **kān** (see), Khasi **khan** (to reflect), Khmer **gan** (look at, enquire)" [G. p. 213]

Trombetti's comparative method

Trombetti provided an illuminating example of his own intuition at work:

"Fijian **kere** (to pray), **kere-kere** (to beg) one day reminded me of Hungarian **kér-** (to pray). The geographical distance between the two languages is enormous, but the comparison was too seductive to simply abandon it. I consulted the magisterial work of Kern...and found corresponding forms...in the languages of Indonesia, Javanese **kere** (to beg), Sumba **kera-i** (to ask, demand).

For the other side of the comparison, I consulted [Donner and Budenz] and found Hung **kér-** (to pray), **kér-de-** (to ask), **ker-es** (to seek), Votj. **kur-** (to pray, demand), Finn. **ker-jää** (to beg), etc....Hung **kér-de-** (to ask) agrees with Yajut **kör-dö** (to seek, desire) Mongol **eri**, from ***γeri** (to seek, demand) and other Altaic words belong to the same root. An important addition was Laz **kor-** (to seek) (inf. **o-koru**, imp. **kori**) and **kor-ap** (to seek out). I

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[14] Trombetti references Comp. Less, 19/244/347

*subsequently added Egyptian **gr** (to seek) and S. Coptic **kōr-š** (to ask for, pray for). Finally, there was Gothic **and-hruskan** (to seek out, investigate). Here we have **hrusk-** from ***kr-u-sk-** = Votj. **kur-y-śk**. The FU verbal suffix, which according to Budenz gives the verb a frequentative meaning, corresponds exactly to the IE verbal suffix –**sk-**. It is notable that in both linguistic groups, this is frequent, precisely in verbs with the meaning of 'asking, praying, seeking). Also the reflexive value, which according to Budenz, FU has, is found in IE, since e.g. Votj. **kar-y-śk** (to be made, make oneself) evidently corresponds to Latin **cr-ē-sc-ō**. These are grammatical affinities of great value, which are not contorted but present themselves in their own right with the character of evidence. The same root of Gothic – **hruskan** is also present in Latin **scrutari** (to examine, search for), from **s-kr-ū**, with the noted prefix s-, and has also been compared with Gk **á-kríbēs** [Ud'O, p. 32-33]*

While he probably worked from his own intuitions, he did set out his methodology:

"The most important methodical rules that we shall observe as far as possible in our grammatical and lexical comparisons are the following:

1. *Compare linguistic groups with each other according to the order of their geographical position.*
2. *Re-establish in each linguistic group by means of internal comparison, the forms and meanings that the words had in their relative Ursprache, or at least relate sufficient material to remove doubts.*
3. *Analyse words to distinguish roots and formative elements, seeking, if possible, to determine the function of these latter.*
4. *Allow for the particular phonetic laws of each language, especially in cases of sharp divergences of sounds.*

But when, with all the methods suggested by scientific method, the original identity of affinity of words or forms belonging to languages from different groups and this resists every proof that we can find, we must nevertheless exclude:

1. *That the identity or affinity is the result of chance;*
2. *That it is the effect of exchange;*
3. *That it can be explained by the fundamental identity of the human psyche."* [Ud'O, p. 26]

The preceding discussion highlights Trombetti's dislikes: explanations based on "the fundamental identity of the human psyche" smacked of polygenesis, he felt that loans were a relatively recent and minor phenomenon since it made no sense to speak of these between the genetically related languages of the early stages of migration, and once the migrations had occurred, geographical remoteness was a barrier to extensive borrowing, hence long-range cognates were far more likely to be due to surviving genetic relationships. On the issue of probability:

"the degree of probability of comparisons is to be analysed on a case by case basis, and one cannot adopt an objective criterion: if we were to have recourse to calculating possibilities, they would tell us that it is ***highly likely*** *that Latin and Greek are of the same origin! Laplace, the great mathematician who perfected the calculation of probabilities had stated that he could bet thousands to one that, if a new planet or satellite were to be discovered, its motion would be direct like all the others. This was imprudent of him, since the four satellites of Uranus and that of Neptune rotate in retrograde fashion around their respective planets. We should thus leave aside the calculation of probabilities. In any case, this tells us that fortuitous linguistic coincidences must be rare, since the possibilities for phonetic constitution are numerous and the number of words grows enormously with the growth in the number of component sounds."* [Ud'O, p. 33]

Another aspect of Trombetti's early views that may raise the eyebrows of modern readers was his dismissal of phonetic laws:

"We should not, however, exaggerate the value of 'phonetic laws'. Above all because there are peoples who perceive and reproduce certain nuances of phonemes that escape us, while there are others that are unaware of differences that for us are extremely large" [Ud'O, p.22].

Even if he appears to have subsequently relented to judge by the large section of his 1923 work devoted to comparative phonology.

In my view, however, the core of his analytical method can only be understood in terms of his views on the origins of language.

The Emergence of Language

"Indeed, primitive language was undoubtedly isolating rather like Classical Chinese; then slowly, certain words within the proposition lost their individuality and independence (and often their accent as well) and became subordinated and united to other words, either as prefixes or as suffixes. In this way, agglutinative languages were born, some of which then became inflected due to the intimate interpenetration of elements of the word. This is the evolution of language in ascending order. But there was also a descending evolution, as we have seen, and on the contrary, it is the only form that we know with certainty." [Ud'O, p. 48

"Inflected languages tend to transform themselves, above all on account of phonetic decadence, into analytic languages...and this is the first step in the return to the agglutinative or isolating stage." [Ud'O, p. 49]

"For different reasons that are not always recognisable, languages derived from the same source may diverge in highly variable ways. Some are conserved in a way highly faithful to the original, while others change profoundly. It may therefore happen that peoples and languages find themselves highly contrasted with regard to the degree of their evolution. There are degraded people who speak languages with a marvellous structure, which are like purple mantles worn by the poor." [Ud'O, p. 49]

The above quotations from Ud'O illustrate the key points of his basic theory of the emergence of language, of which he conceived as a single great cycle that moved from analytic to synthetic and back. In this, he made a leap of faith in assuming that the first stage had been more or less completed by the end of the unitary period, and that all we would ever observe in surviving languages was the return process.

This implied that any word in a surviving language had to be considered as a potential composite of root + theme + inflection[15]. He postulated that the original root would be of V, CV or CVC type, admitting that 'verbal' roots probably had an original aoristic meaning which could be modified by ablaut, reduplication or merger with other roots.

At the same time, he seems to have gradually softened his view that the behaviour of roots during the unitary period was invisible to modern eyes:

1905:
"*The original meaning of predicative roots was certainly always highly material, but also at the same time indeterminate to a high degree, not only with regard to the grammatical function, but also with regard to extension, in analogous fashion to what one observes in child language, in which, e.g.* ***nanna*** *means not only 'sleep' but also everything that referred to sleeping, such as the bed or the cradle, cushions, etc. And here it is important to distinguish between what a word genuinely expresses and what it can mean.*" [Ud'O, p. 60]

1908:
"*We should strictly expect to find a single demonstrative pronoun....but in the Bantu languages which are more archaic, the demonstrative varies according to the class of noun that it accompanies or represents. Primitive expression was more concrete than ours. Therein lies the reason for the plurality of demonstratives*" [Pron. 351-54]

1923:
"*The radicals of action verbs are of onomatopoeic origin...I cannot conceive of another origin*" [G. p. 227].
"*Words are subject to changes in meaning so large that it is often difficult to follow their course back to the origin...I shall show with an example how one can arrive at an onomatopoeic root where this would seem to be impossible. In [Comp. Less. 372] I documented a type* ***bu, pu*** *(hairs, wool). What is the origin of this monosyllable? Undoubtedly, [it] is to be sought in the extremely widespread onomatopoeic root* ***bu, pu*** *(to blow).*" [G. p. 234].

Trombetti also proposed that different vowels could express different concepts of space or quantity, e.g. Tamil ***ivan*** (this), ***avan*** (that) ***uvan*** (that over there), with this ***i/e-a-u/o*** series echoed in Hung. ***innen*** (here), ***onnan*** (there), Nama ***né*** (this), ***-nā*** (that one by you), ***noú*** (that one over there).

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[15] Trombetti explictly refers to the Indian model: dhatu + pratyaya + vibhakti [Ud'O p. 51]

This idea was already present in his 1905 discussion of ablaut and personal pronouns, but in his 1923 work he gave it much more emphasis, articulating the concept of **polarity**, by means of which "*through antithetic variations in the same word, language expresses in the most natural way the two opposed aspects of the same thing.*" [G. p. 235].

Rather than simply regarding these vowels as space markers in their own right, however, Trombetti saw them as reflections of universally occurring roots ***i*** (motion from) and ***u*** (motion towards), which began as interjections and became the verbs 'go' and 'come'.

Their original meaning was still reflected in numbers (1 = "this one", 2 = "this one that one"), e.g. gr. Boa ***li-ru*** < ****li-lu*** (two), Papuan ***li-lo***, Nifilole ***li-lu***, and possibly Gk ***di-du*** > ***δίδυμος*** (twin).

As well as in first and second person pronouns:

Bushman	***ii*** *(us = these ones)*	***uu*** *(you = those)*
Pul	***en, enen, eden*** *(we)*	***on, onon, odon*** *(you pl.)*
Avar	***niž*** *(we)*	***nuž*** *(you)*
Tamil	***en-*** *(I),* ***em-*** *(we)*	***un-*** *(you),* ***um-*** *(you)*[G. p. 240]

Indeed, as early as 1905, he had shown that the first and second person pronouns, ***mV/Vm***, ***nV***, ***Vk***, etc. were actually composites in which the signifying part was the vowel:

"*In all the preceding forms, the essential element is **a** or **i**, while the prefixes have a deictic value similar to our own 'ecco' (Egyptian **yn**, Latin **ēn*** (see! behold!) *etc.), and the suffix **K** is also present in the second person pronoun. If, therefore, the N element is not essential, the doubt emerges as to whether even the M element....is essential. Indeed, Bantu has **mi** next to **ni**, and if the latter is **n-i**, then the former will probably be **m-i**. In Dravidian, the essential element may perfectly well be represented as merely **-ā-** or **–a-**, as Caldwell already supposed. Also in IE, the real theme appears to be **e**, cf. the nominative **é-go** and the theme **e-me** (Hung **é-n**, etc.). The Bantu forms **m-i** and **n-i** can be easily explained: it is enough to remember how nominal prefixes containing m or n, i.e. **m-a**, **m-i m-u** and **n-i** correspond to verbal prefixes without m or n, i.e. **a**, **i**, **u**, **i**, respectively.*"

"*But what is most notable is that the same element **M** is present here and absent these in the first and second person pronouns also in Malayo-Polynesian*"

"*Extremely important is the note by Rev. H. Roberts, A Grammar of the Khasi language that "In the valleys to the West and in Jiantia to the east" **ma-i** is used for **nga** (I), **ia-i** for **ia-nga** (me), **na-i** for **na nga** (from me)*"

"*The root of the first person pronoun is thus **a**, **i**....I suppose that **a** as originally an element of an interjectional nature, all the more so since Hamito-Semitic allows us to reconstruct a form **ah**. As for the element I, this is undoubtedly the noted demonstrative 'here, this'. The primitive meaning of Semitic **kalb-ī** (my dog) must have been 'this dog here by me'...In analogous fashion, we might understand **kalba-ka** as 'that dog by you'.*" [Ud'O, p.82-83][16]

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[16] Greenberg evidently had the same idea in Indo-European and its Closest Relatives, p. 82/83.1, but did not go as far as Trombetti in his analysis.

At the same time, the main thrust of language development in the unitary period took the form of grammaticalisation of other words which became themes and inflections.
"However, after the roots, the oldest elements are the themes, which are usually much more durable than inflected forms. Hence it is among the themes that we must seek the oldest grammatical formations, which may be almost latent and petrified. This is the true field of linguistic palaeontology." [Ud'O p. 53].
"Often attention is given to the most conspicuous and external affixes, but in my opinion, the oldest are the vowel suffixes immediately in contact with the root, which more easily pass unobserved. Many suffixes are composites or appear as amplifications of prior suffixes" [Ud'O p. 53].

This insight has two powerful methodological implications: firstly, while not as old as basic roots, themes and possibly inflections should at least date back to the end of the unitary period and hence are suitable 'tracers' for mapping the subsequent differentiation of languages, and secondly, these 'tracers' may themselves be composite so that cognates between languages and the underlying process of differentiation may be susceptible to further analysis by comparing subcomponents of themes and inflections.

It is impossible to do justice to the wealth of material presented firstly in Ud'O and then greatly expanded in the Morphology section of G. I shall thus limit myself to a couple of examples to illustrate the observation that Greenberg's last work[17] on Eurasiatic largely returns to the kind of analysis that Trombetti had done almost a century earlier:

Accusative/Locative –M
*"For IE, ***-m** is reconstructed as accusative singular for masculine and feminine nouns and in neuter thematic stems for both the nominative and accusative singular. In Uralic, ***-m** is reconstructed for the accusative and is found in all three branches, Finnic, Ugric and Samoyed. It is generally singular, but in Cheremis, Vogul, Selkup and Kamassian, it also occurs in the plural and in the dual, where those categories exist…Evidence for an **–m** accusative is found in two branches of Altaic, Mongolian and Tungus. In Classical Mongolian, there is a defective third person singular pronoun that only occurs in the genitive **i-nu** (for **i-** see N^{0}. 8)* [Greenberg, p. 129].
*"In a number if IE oblique nominal cases, largely parallel forms with both ***m** and ***bh** occur"* [Greenberg, p. 139].
*"It was noted under N^{0}. 24, the **–m** accusative, that the IE and Uralic accusative might have originated from locative **–m**. In languages like Latin, the accusative is used for time periods or for spatial extent. In Walde…**tam** (for such a distance)"* [Greenberg, p. 143-44].

Trombetti 1905:
*"**M-** the accusative more often than the nominative has a special sign. The most widespread suffix is **–ma**, **-me** and generally **–m**: IE, suffix of the accusative singular **–m**. Also in Finnish and Lapp **–m** is limited to the singular, but in Ceremis and Vogul the same suffix is also found in the plural."*
*"It seems to me that the case in **–m**, **-p** of Eskimo was originally an accusative. This also has correspondences in other languages. As for the Turkic languages, the **–m** was*

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[17] J. Greenberg, Indo-European and its Closest Relatives, Stanford, 2000.

transformed into an ***–n****, whence* ***–ni****, etc. Also in the Dravidian languages, the* ***–m*** *was transformed into an* ***–n****, but was conserved in old Canarese. Even in Mitanni, Pre-Armenian and Arzawi*[18] *it appears that* ***–n*** *was the sign of the accusative. In some Indochinese languages, such as Lepcha and Dophla*[19]*,* ***-m*** *is conserved as a sign of the accusative. In S. Chinese, the suffix is* ***–ni****."*
"The use of ***M*** *to form the accusative is thus extremely widespread, and we could also compare Bantu* ***u-****, the third person subject pronoun with* ***–m-u-*** *the same object pronoun. But* ***M*** *has other functions."*
"We would also compare Georgian ergative ***–ma-n****, Svan* ***–e-m****"*
"The Semitic mimation [sic] is basically the same element", cf. Assyr. ***atta-ma****,* ***attam*** *(you sing) with OInd* ***tvá-m****, Assyrian* ***mūša-mma*** *(at night) with OInd* ***nákta-m****. From an accusative* ****so-m*** *(he himself) may be derived IE* ***so-m-ó*** *(the same, equal), □Gk homos, cf. Drav* ***ta-n-*** *, Plural* ***ta-m-*** *(the same) and Assyrian* ***šū-ma****"* [Ud'O p. 129]

Trombetti 1923:
"*I think it is opportune to unite these two suffixes [****-p/b*** *and* ***-m****] since they change places with each other in all groups and are equivalent."*
"***-m*** *accusative requires an observation, which I do not believe should be separated from the other cases. In Bantu, the third person of the* 1^{st} *class when united to the verb has two forms* ***ó-*** *for the subject and* ***-mó-*** *for the object, which I explain from* ***mó-ó*** *(towards him/her). Similarly Pul has* ***o*** *(he/she), object mo from* ****m-a-o*** *(towards him/her), cf* ***ma*** *(you/to you sing.) from* ****m-a-a*** *(towards you). I therefore consider that in any case, the accusative* ***–m*** *contains a preposition.*"
"*IE* ***–m*** *for the accusative singular and* ***–mi*** *for the instrumental...and the duplicity manifests itself in so far as next to* ***–mi*** *we find* ***–bhi****, cf Arm,* ***mar-b*** *from* ***mair*** *(mother),* ***gailo-v*** *from* ***gail*** *(wolf) and Gk* ***ī-fī*** *(with force),* ***theó-fī****.*" [G. p. 682-83].

Trombetti also cites extensive evidence for dative/locative particles of the **pi/bi** variety in Indochinese and American.

41. Adverbial Participle P

"There is in Eurasiatic a widely distributed element ****pa*** *(more rarely* ****pi****) that is usually suffixed to verbs. Its probable original meaning is well illustrated in Turkic, where Menges describes it as an "expression of successive actions, whose time levels are not essentially different or distant from each other"*
"With such a form as the probable starting point, a number of developments, syntactic and semantic, can take place. The adverbial participle can become an ordinary participle ('weeping he came' becomes 'he, the weeping one' came). ***Moreover, simultaneous or nearly simultaneous action easily takes on a causal or conditional nuance.****"*
"In all branches of Uralic, there is a participle in ****-p*** *or* ****-pa****.... In Finnish, in addition to forming adjectives like* ***elä-vä*** *'living', it forms the third-person singular and plural of the verb, e.g.* ***tule-vä-t*** *'they come"*

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[18] Trombetti appears to be referring to Hittite or a related Anatolian language.

[19] Dophla appears to refer to Dolpo, a Tibetic language.

*“Menges (1995: 135-36) connects Turkic **–p/-Vp** (also pan, ban in the Uighur inscriptions) with Tungus **–pi**, **-fi**, as well as (following Räsänen), the Uralic present participle in **pa** ~ **pä**.” [Greenberg, pp. 175-77]*

Trombetti 1923:

*“Among causatives, the element **–p** is extremely widespread”*

*“In [Caucasian], I find Laz **wo-gna-re** (I know) > **wo-gna-pa-re** (I make known)”*

*“In IE, forms such as Skt **sthā-p-áya-ti**, causative of **sthā-** (to stand), Lith **stā-pī-ti** (refl. Be quiet)... That is, **-p** is a ‘determinative’, of which other examples: IE **ter-p** in Latin **trepidus** and Slav **trepetŭ** (tremor); **kle-p-** in Lat **clepo**, Gk **kléptō**, Goth. **hlifa** (I steal); **sne-p-** (to bathe), **ser-p-** (to crawl).”*

*“In the FU languages, -p forms verbs with a momentary or inchoative meaning, e.g. Hung **ālla-p-** (to kill), Finnish **käü-pä** (going), Veps **elä-b** (living) c.f. Celtic **mar-b/mar-w** (dead)”*

*“Denominative: Finnish **liha-va** (fleshy), **vere-vä** (bloody), cf. Skt **kēša-vá-** (long-haired), **pad-váán** (having feet), Georgian **c’qlo-va-ni** (watery).”*

*“In the Altaic languages, we find **-p** and **–b** as ‘determinatives”, e.g. Mongol **gil-be-** (to shine, sparkle),...**yazup** (having written), Manchu **–fi** intransitive, **-pi** past gerund: **ara-fi** (having written), **elde-pi** (having shone).”*

*“In Bahing, every verb can be made causative by adding **pāto** (do), e.g., **gā-pāto** (to cause him to eat); Tibetan **–pa/–ba** participle and infinitive, e.g. **r-god-pa** (laugh), **dro-ba** (be hot)”*

*“In Uraloaltaic, Indochinese and American we have **p-** with a causative value.*

*“Formosa **pa-ita** (to cause to see), Nabaloi **pa-bunu** (to cause to be killed), Bugis **pa-pole** (to cause to come) Tar. **pa-akan** (to cause to eat), Kambera **pa-laku** (to cause to go) Angkola **pa-uli** (to make beautiful), Hova **mam-pa-turi** (to cause to sleep). Melanesia **va-**, N. Guinea **pa-**, **ba-**, **va-**”*

*“Mon **p-tim** (to cause to know), Khmer **p-riën** (to teach)”*

*“Chiapanec **la-w’i** (to die), **pa-w’i** (to kill), Chibcha **šike-n** (to become dry), **b-šike** (to dry), Kariri **pe-baaha** (to cause to sin), **podzo** (to wake up-refl.), **pe-podzo** (to wake-trans.)” [G. pp. 735-737]*

Trombetti evidently shows that the there is nothing specifically Eurasiatic about Greenberg’s suffixes.[20] The fact that Greenberg quotes extensively from linguists contemporary with and even prior to Trombetti without ever mentioning the latter’s name, seems to confirm Ruhlen’s remark that Greenberg was unaware of his work.

Conclusion

It is hard to do justice to the wealth of material in Trombetti, although I hope that I have succeeded in sketching a portrait of the man and his key ideas, and in making it clear that he is still relevant for two reasons. Firstly because his (possibly still) unparalleled knowledge of languages and data-oriented approach made him a largely objective observer, and secondly (in my view, his great achievement) because his views on the origin of language provided him with a powerful methodological tool for highlighting long-range cognates, particularly morphological ones.

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[20] e.g. Greenberg himself cites Miwok pa- ““indirective (i.e. to get someone to do something)” and was thus certainly aware of the presence of this adverbial/causative participle in Amerind, but regrettably never published the grand synthesis that was so easily within his grasp.

While his classification may yet be shown to stand, the new genetic evidence pointing to a single 'out of Africa' migration seems *a priori* not to support his notion of an Indian homeland, raising the obvious question as to whether we can refit the linguistic relationships he proposed to this new evidence.

My own preliminary attempt would go like this: Trombetti is telling us that Proto-World would have been closest to Bantu and this would have been spoken by the first emigrants from Africa, whose language evolved into the Munda-Polynesian group. At the same time, a second linguistic group had emerged from Bantu that subsequently split apart into Afroasiatic[21],[22], and Dravidian-Australian, with the latter branch overtaking the Munda-Polynesians on the route to Australia and Papua New Guinea, perhaps because the former had been displaced from Indonesia by the Toba supervolcano that erupted around 74,000 years ago[23]. There is a nevertheless a knot to be untied here, since the genetic evidence for Sino-Tibetan suggests that the route into Tibet and Burma was via the Bay of Bengal, while Trombetti's linguistic evidence links this family (what he would have called 'Indo-Chinese') to languages spoken in Western Eurasia: Caucasian, Sumerian and ultimately Afroasiatic, while the link to Dravidian was more distant. The implication is that his Boreal languages were spoken by the populations who 50-70,000 years ago had moved inland from the coast to form a Sprachbund stretching from Mesopotamia to the Bay of Bengal with pre-proto-Sino-Tibetan at the extreme Eastern terminus, pre-proto-Amerind around the Indus Valley and pre-proto-Eurasiatic to the West, with a relatively early move into Western Central Asia and Western Siberia (Uraloaltaic?). If proven, such notions would evidently turn modern views of time depth (although emphatically not Trombetti's own) upside down. I hope that this essay will take a first step towards this by lifting his work out of the darkest and most undeserved obscurity.

With thanks to Mario Alinei, Merritt Ruhlen, Hal Fleming and Stephen Oppenheimer for their comments. The author assumes responsibility for all opinions, translations and associated errors, although notes that bibliographical rigour is not one of Trombetti's many virtues.

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[21] It should be remembered that Trombetti classified the Hottentot/Bushman languages as Southern Hamitic.

[22] "Where Hamito-Semitic departs from Bantu, it generally takes the form of innovation, except for the plural by doubling." [G. p. 53]

[23] This does not imply that he thought that Munda-Polynesian and Dravidico-Australian were unrelated, only that the relationship was more distant. Indeed, Gatti includes a series of Australian/M-P cognates.

Some Comments in Trombetti by a Modern Long Ranger

We regret that, because of time constraints, our original plan to handle the Trombetti paper in the MT*Treatment format was not realized. Malheureusement, we will have to make do with only one person's opinion. Nothing should prevent long rangers from writing us about their thoughts on the subject of Alfredo Trombetti and his contributions to our joint enterprise. All such comments will be published in Issue XI, unless they are so numerous that we are forced to publish a special issue devoted to Trombetti. (By Harold C. Fleming)

Despite key contributions made from time to time by English, Russian, Czech, and Scandinavian scholars, linguistics and its inner core, historical linguistics, was dominated by **German and French** scholarship for most of the 19th century and much of the 20th. After World War II along with the new American imperium came Bloomfield, Gleason, Harris, Swadesh, Greenberg, and Chomsky and many others. But forgotten by all was a great **Italian** linguist, one of the few who might truly be called a **genius** (Sprachbegabung, genio) and one who came close to realizing Charles Darwin's notion that a proper **classification** of life forms or humanity would be **genealogical**, on the model of language.(Cf *The Origin of Species*, 1958: p.392, Mentor Edition.)

Many years ago my good friend and colleague, **Lanfranco Ricci** (Univ. of Napoli) told me he was surprised that I knew about Trombetti –though all I knew was his name and reputation as a pioneer. Ricci said that when he was a student "we were taught to be ashamed of Trombetti or at least not to hold him in high regard." Why? Because Alfredo was off-beat, irregular, not properly trained and a wild speculator. (These are not exact quotes; I remembered the **gist** of what my friend said.) This reminded me strikingly of **Morris Swadesh** who had the same thing done to him. Or Greenberg nowadays.

Reading Jonathon Morris' excellent account of Trombetti's life and work, one cannot help recognizing things that we were never taught in university but we have come to appreciate on our own. I am not going to try to specify all those ideas but, suffice it to say, his entire viewpoint on how to proceed as historical linguists and as human scientists is so similar to the attitudes of long rangers and historically-oriented anthropologists as to make one suspect that Trombetti had given a course somewhere and we all had taken it! He sounds so **modern,** yet his pronouncements as portrayed by Morris were completed the year Noam Chomsky and I were born, with Joe Greenberg still in grammar school.

Must we say something critical about Trombetti's work? Okay, it is fairly easy to do because he was over-extended relative to his data base. At least in Africa his taxonomy was wrong or at least his names for some taxa were mistaken. Some of his uses of Somali and 'Galla' (Oromo) data were mistaken and he made several major errors vis-à-vis Niger-Congo and Afroasiatic (Hamito-Semitic). Thus he put two phyla, Niger-Congo and Nilo-Saharan into one rubric, **Bantu**, which was about as inappropriate as you could get; like naming Austronesian and Austro-Asiatic after Polynesian. Trombetti's insertion of Khoisan languages into Hamito-Semitic suggest the reason for his screwed up African taxonomy –he was following the dominant Africanist of his day, Carl Meinhof, whose typology-derived taxonomy created a mess which Greenberg labored for years to correct.. Remember too that Trombetti had precious little genetics or archeology to help him out and paleoanthropology was just reaching puberty.

New Materials on the Kusunda Language[1]

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General Background:[2] The Himalayan kingdom of Nepal is extremely rich and complex in cultural as well as linguistic diversity. This diversity is the result of the coexistence there of diverse ethnic groups for thousands of years, each of which has its own distinct language and culture. Kusunda is one of the ethnic groups whose language and culture are valuable to the students of ethnology.

The Kusundas of Nepal feel embarrassed at being identified as Kusunda. Therefore, they seem to have shifted their identity to other languages and cultures, apparently leaving an impression of their extinction. Their tribal name is *Myahak,* 'king of forests.' I quote here the former British Resident Representative to Nepal, Brian H. Hodgson, on the Kusundas:

> They were generally supposed to be autochthones, or primitive inhabitants of the country, were near to what is usually called the state of nature as anything in human shape can well be, deemed very precious by all the real students of ethnology. Their origin, condition and character are, in truth, ethnic facts of high value, as proving how tribes may be dislocated and deteriorated during the great transitional eras of society (Hodgson 1857).

This information is brief and sketchy but it has always induced me to go into Kusunda studies. Kusunda research is not yet completely accomplished. It will take some more time, and therefore we hope that our Kusunda informants will live long enough that we will be able reasonably to finish with our studies on them. It is our sincere hope that the concerned authorities will also do something meaningful to preserve Kusundas in the Himalayas.

Kusunda has also been cited as a dead language. My research data on Kusunda do not support this claim. It is a fact that Kusunda has quite a few speakers who have shifted to other language groups, causing language attrition owing to marriage, migration and other socio-economic changes the societies have undergone. Under these circumstances it may well be surmised that Kusunda is on the verge of extinction and may die out with the death of its remaining living speakers. It is, therefore, high time to document and analyze this language before it is lost to oblivion.

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[1] Presented to the Fourth Harvard Round Table on the Ethnogenesis of South and Central Asia, Harvard University, Cambridge MA, USA. May 11 - 13, 2002.

[2] Before presenting anything about the Kusundas, I must extend my profound gratitude to Dr. Michael Witzel, Wales Professor of Sanskrit at Harvard University, without whose constant guidance and encouragement for almost two years (*via* electronic media) my presence to this prestigious conference would have been impossible. I should also sincerely thank Professor Harold Fleming and Paul Whitehouse, whose inspiring letters prompted me to continue my research on the Kusundas. Paul Whitehouse's article in Mother Tongue (1997) was also something new and important to me. And his comments on my article in *Janajati* (2001) also inspired me to further work on the Kusundas.

Kusunda Ethnicity and Population: Kusundas are also called *Banarajas* 'kings of the forest', because they used to live in the forests. Kusundas called themselves *Myahak*[3] and they had a kind of taxation system over the Rautes.[4] Kusundas were kings and Rautes were their subjects. Generally, Rautes run away if they happen to see a Kusunda from a distance. This can be noticed even today along the Raute track in the Surkhet district of Midwest Nepal. Kusundas had a foraging culture. But the case is different now; there is no Kusunda left who gathers and hunts in the wild. I have found seven Kusundas in the central and mid-western hills of Nepal whom I believe to be ethnically pure by origin, and two of them are younger females married to Chhetris,[5] another ethnic group in the country. There are few other Kusundas of mixed origin; including them the Kusunda population in Nepal will not exceed fifty in total.[6]

I believe both "Banaraja" and "Kusunda" are names given to the *Myahak* peoples (Kusundas) by other communities. Kusundas are said to be the offspring of Kusha, Rama's second son, born from *kusha* grass in Valmiki's Cottage. This story is well depicted in the *Ramayana*. The Chepangs[7] also believe they are the offspring of Sita's first son Lohari or Lava who is also very famous in the *Ramayana*. Lohari and Kushari were two sons of Sita. The Kusundas believe that they are offspring of Kushari - Kusha. Later Lohari and Kushari became rivals. Then the Kusundas and Chepangs began to live separately. Some of the Chepang words have some similarity with those of Kusunda. Both Kusunda and Chepangs are found in the hills of Nepal.

Kusunda Language: Kusunda culture is now nonexistent. But their language remains, which, I believe, originated in the Sino-Tibetan area; or it could be an earlier language in this area. However, a number of eminent linguists have written to me explaining that some of the apparent Kusunda cognates with Tibeto-Burman languages may instead be borrowings. If this is to be believed, then Kusunda appears be a 'barren' language without its own native words for objects such as: *ing* 'sun', *ngsa* 'fish', *uyu* 'blood', *gepan* 'language', *ung* 'trail', *langhai* 'village', *suta* 'thread', *mucha* 'banana', *kakchi* 'crab', *tu* 'snake', etc. This is a matter for thorough research. Robert Shafer (1954) was the first scholar to identify Kusunda as a language isolate. Professor H. Fleming as well as most other linguists also believe that this is a language isolate. Yet, it may also be argued that Indo-European, Tibeto-Burman as well as other languages have shared words with Kusunda. It is a matter of serious study as to what is the genesis of Kusunda language in the Himalayas. The Kusunda people, their language and culture are very important to linguists and anthropologists alike. Recently the Kusundas have undergone a drastic change in their life style, as the result of which they have completely forgotten their own culture and tradition. Still, fortunately, there is the language living at the moment. This language has not yet been well studied for we have obtained only limited data so far.

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[3] *Myahak* is the indigenous tribal name of the Kusundas. Note that the Gurungs have *Tamu*, the Shaukas have *Rong*, and Limbus have *Yakthung* as their indigenous tribal names.

[4] The Raute are another ethnic group that live in the jungles of West Nepal even today They speak Khamchi, a Tibeto-Burman language, and they have clan names (*Shahi, Sen, Thakuri,* etc.) just as the Kusundas. Their females are not socially free (*e.g.,* cannot speak with unrelated men or move freely) and are treated differently than in the Kusunda community, where women are socially much more free.

[5] *Chhetri* is cognate with Sanskrit *Kshatriya,* the warrior class of ancient India. They speak Nepali, the major Indo-Aryan language of Nepal.

[6] Kusundas and Rautes have been found taking *Thakuri* surnames such as Sen, Singh, Saha, Malla etc., mainly to uphold their social status. If only a few of them are "Upgraded Kusundas" then the Kusunda population would increase appreciably. The Thakuri population in the last national population census (1991) was 1.62 % of the total 18,491,097. (The Thakuris of Nepal are generally of mixed origin: Brahmin [Indo-Aryan] father + ethnic [non-Indo-Aryan] mother.) When addressed abusively Thakuris are also called 'Kusundas' by other peoples. The present Shah King dynasty belongs to the Thakuri community. In the coat of arms there is a picture of a hunter with a bow and arrow in his hands. The Kusundas have the word *tut* and *mui* for 'bow' and 'arrow' respectively.

[7] Chepangs are another ethnic group in the central hills of Nepal. Hodgson had found them 'few degrees above the Kusundas'. Nevertheless, a few of them can still be found in caves. They are doing better in recent years.

Additionally, the SIL *Ethnologue's* citation of the death of the Kusunda language has dispirited linguists from finding other Kusunda speakers and studying the language. Under the auspices of His Majesty's Government of Nepal, I was able to go for some research and find a few Kusundas who could speak the language fluently. A month ago [as of May, 2002], I was informed that there is yet another male Kusunda who can speak the language. I hope to see him soon. Thus, there are still ample opportunities for every one of us to study the language and understand its importance.

Hodgson - Grierson Data: When talking of the Kusundas we happen to remember Hodgson. Having lived in Nepal for a long time in the early nineteenth century, Hodgson had been very fortunate to go into studies on languages, literatures and religions of Nepal and Tibet. He was much fascinated by the ethnic, linguistic and cultural diversity of the Himalayan region. His works on these areas are always great. But as concerns the Kusundas he could not personally meet with them and has so admitted: "During a long residence in Nepal, I never could gain the least access to the Kusundas, though aided by all the authority of the Durbar" (Hodgson 1957). In those days Nepal was experiencing certain political changes – the Rana Regime had recently been installed and lasted for 104 years; under them, there were no educational nor other sorts of developments. It is therefore understandable that Hodgson's informants were people from other communities. It is also possible that those informants were not even from the speakers' neighbouring community and had very little knowledge of Kusunda as well as of Tibeto-Burman languages in Nepal. Needless to say, some of the Kusunda data obtained in that way now require verification.

Grierson drew on Hodgson's vocabulary for the Linguistic Survey of India in 1909. Later scholars also have drawn from the latter's work. These data have to be independently verified again. Below is a comparative listing of the Hodgson data, drawn from the Linguistic Survey of India:

English	Kusunda (new data)	Kusunda (Hodgson data)
one	*kasti*	*goisang*
two	*dukhu*	*ghinga*
three	*dahat*	*daha*
four	*pigo*	*pinjang*
five	?	*pangang-jang*
he	*git/gina*[8]	*gida*
hand	*nabi/amokh*	*gipa*
tooth	*ouhu*	*toho*
eye	*ining*	*chining*
child	*ghichi*	*gitase/chyachi*
good	*ohin*	*waiyaki*
house	*wohi*	*bahi*
trail	*ung*	*won*

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[8] *git* = 'he' (nominative); *gina* = 'his' (possessive).

Reinhard-Toba Data: John Reinhard and Sueyoshi Toba also worked on Kusunda some 32 years ago. Their data are the primary data recorded by Reinhard from the field, which were later analyzed by Toba in Kathmandu. But the latter had not been able to see and speak with the Kusundas in person. Both of them were non-native researchers. I have found a certain amount of redundant data in the Reinhard-Toba lists; nevertheless, it is a scientifically accomplished work. Reinhard has honestly admitted that:

> This [Kusunda analysis] unfortunately was based on very little data, is incomplete and contains several errors; significant variants obtained from different informants have been listed. Several of these terms could not be checked and therefore the list should not be considered definitive. (Reinhard 1976)

Therefore, there are also some inevitable redundancies.

Similarities with Other Languages: Having found some sorts of similarities with a few indigenous languages of the Tibeto-Burman family, I therefore believe that Kusunda originated in the Sino-Tibetan area. Kusunda *mahi* 'water buffalo' and *mai* 'mother' are similar to Central Magar *mahi* and *mai*, with the same meanings; cf. also Sanskrit *mahisha*. Kusunda *mai* is quite close to Sanskrit *maataa* meaning 'mother'. Some other Tibeto-Burman linguistic communities also have *mai* for mother.[9] In the same manner Kusunda and Magar say *suta* for 'thread', and its Prakrit form is *sutta* and in Sanskrit it is *suutra.*[10]

I have already mentioned above that some linguists differ with my view on the origin of Kusunda. They believe that Kusunda is a language isolate – not sharing recent common origin with any languages. But my recent findings confirm that Kusunda has noticeable affinities with a number of indigenous languages spoken across the northern belt of Nepal. Therefore, it is possible that this language originated in the Sino-Tibetan area, and that other major language families also shared words with it. Below are some sample cognates.

1. one
KUSUNDA: *kasti* : TIBETO-BURMAN: *kat* (Central Magar)
2. blood
KUSUNDA: *uyu* : TIBETO-BURMAN : *chyuhui* (Baram), *uyu* (Chepang), *hayu* (Dura)
3. trail
KUSUNDA: *ung* : TIBETO-BURMAN: *ungma* (Baram)
4. fish
KUSUNDA: *ngsa* : TIBETO-BURMAN: *ngyasya* (Western Magar), *ngya* (Chepang), *dishya* (Central Magar), *dishya* (Dura)
5. fire
KUSUNDA: *za* : TIBETO-BURMAN: *chhawo* 'warm/hot' (Tibetan)
6. language
KUSUNDA: *gepan* : TIBETO-BURMAN: *ge+pang* (Western Magar *ge* 'we/our' + *pang* 'language'), *ke-gepa* (Tibetan: 'you cry aloud')

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[9] Proto-Sino-Tibetan **maaH* 'mother' > Tibetan and Chepang *ma,* Garo and Kanauri *ama,* etc. (Peiros & Starostin, *A Comparative Vocabulary of Five Sino-Tibetan Languages,* 1996). Of course, words of this type are found all around the world, and can be attributed to Proto-Human. [Ed.]

[10] This word is very clearly of Indo-Aryan (Indo-European) origin, from the root **syuu-* (cf. English *sew, seam,* etc., *suture* < Latin *suutuura*). [Ed.]

7. banana
KUSUNDA: *mucha* : TIBETO-BURMAN: *mocha* (Central Magar); *moje* (Tamang), *muja* (Dura), *mach* (Gurung); also in Dravidian (Tulu *mote,* etc.)
8. water buffalo
KUSUNDA: *mahi*: TIBETO-BURMAN: *mahi* (Central Magar), *mai/maikha* (Dura), *mai* (Gurung); cf. Skt. *mahisha*
9. village
KUSUNDA: *langhai* : TIBETO-BURMAN: *langha* (Central Magar)
10. sun
KUSUNDA: *in/ing* : TIBETO-BURMAN: *nin/nim* (Nymba), *nima* (Tibetan), *nyam* (Chepang)
11. bread
KUSUNDA: *mangmi* : TIBETO-BURMAN: *mangmi* (Bhote)
12. mother
KUSUNDA: *mai* : TIBETO-BURMAN: *mai* (Central Magar/Western Magar), INDO-EUROPEAN: *maataa* / *maatar-* (Sanskrit)
13. forest
KUSUNDA: *gelang* : SINO-TIBETAN: *bling*
14. thread
KUSUNDA : *suta* : SINO-TIBETAN : *suta* (Central Magar) ; INDO-EUROPEAN : *sutta* (Pali/Prakrit); *suutra* (Sanskrit)
15. crab
KUSUNDA: *kakchi* : SINO-TIBETAN: *khakre* (Tamang)
16. snake
KUSUNDA: *tu* : SINO-TIBETAN: *du* (Bhote); *pu* (Kulung Rai)
17. egg
KUSUNDA: *gwa* : SINO-TIBETAN: *wa-kun* (Chepang); *wadi* (Kulung Rai)
18. monkey
KUSUNDA: *guinyau* (CN), *haku* (MWN) : SINO-TIBETAN : *laku* (Dura)
19. nose
KUSUNDA: *inau* : SINO-TIBETAN: *nu* (Dura)
20. leg
KUSUNDA: *yen/yeng* : SINO-TIBETAN: *lung* (Kulung Rai)
21. louse
KUSUNDA: *kee* : SINO-TIBETAN: *see* (Kulung Rai)
22. goat
KUSUNDA: *miza* (CN), *azaki* (MWN) : INDO-EUROPEAN: *aja* (Sanskrit)

The list above shows that Kusunda has some kind of relationship with other languages across Nepal. Therefore, this sort of relationship should not be taken as borrowings only. It is also a matter of deep study as to who borrowed from whom. Below I give some further explanation of the language:

(a) There is prominence of the nasal /ŋ/ sound in the Kusunda language, and one of the striking characteristics of Tibeto-Burman languages is that they have nasal /ŋ/ prominently occurring in all distributions; for example, Kusunda *ngsa* 'fish', *ngyangdi* 'woman', *dimtang* 'beer', *langhai* 'village', *ung* 'trail', *gelang* 'forest', *ing* 'sun', *mangmi* 'bread', *sijang* 'beer', etc.

The Santhal language (belonging to the Austro-Asiatic Munda family has */yeŋ/* prominently occurring in its major word classes.

(b) For 'fish' the Kusundas say *ngsa* [ŋ+sa], the Magars of the Karnali area say *ngya+sya*, the Chepangs *ngya* or *nga*, the Barams *nanga* and the Magars of the Gandaki area [di+sya]. These segments [ŋ+sa], [nga+sya] and [di+sya] have the same meaning and the formation of these words are also distinctly similar. The Kusunda [ŋ+sa] has **n* of *nur* or *ngr* for water and **sa* for meat. Fish is 'meat from water'. Therefore *ngsa* is a Tibeto-Burman word.

(c) Concerning numerals in Kusunda there is *kasti* for 'one' and *pigo* for 'four'. In the Magar language of Central Nepal these are *kat* and *buli* respectively. In the Baram language 'four' is called *bi*. The Kusunda *pigo*, Baram *bi* and Magar *buli* have bilabial similarities in common. In Kusunda counting does not exceed 'five': *kasti* for 1, *dukhu* for 2, *dahat* for 3, *pigo* for 4 and *pangang-jang* for 5. The case is very similar to the Magar language: *kat* for 1, *nish* for 2, *song* for 3, *buli* for 4, *bang* for 5.

Complex Pronominalization: Kusunda is a grammatically complex language, one feature being pronominalization. The Hodgson-Grierson data and the Reinhard-Toba data, which most recent day linguists have utilized, must be reanalyzed. The Reinhard-Toba data seem to have been obtained more scientifically than Hodgson-Grierson's. But the former's data (Reinhard-Toba) have also been found to contain some flaws. My informants have sometimes given me different data. Therefore, we should also check other sources, along with these two, before we draw any conclusion. The Kusundas have a habit of answering someone's question just in one word or two, which is a common feature in other languages of the Tibeto-Burman family. For example, in Kusunda:

taman	'(I) eat'
itanan	'(I) give'
kamaji	'(it) bites'
chii chimat	'my stomach'
nii nimat	'your stomach'
gidi gimat	'his stomach'

I was able to record some Kusunda sample sentences a few months ago. These sentences are collected from two female Kusunda speakers of the Rolpa and Dang districts, in mid-west Nepal. One of the speakers' daughter, who is married to a Chettri, can also speak the language. While at home mother and daughter converse in the Kusunda language. I found Kusundas have a habit of speaking only one word or short phrases when speaking to others.

I eat rice	*chi kadi gaman / kadi tamdi*[11]
I go home	*chi woha / wohi tagai*[12]
I come home	*chi woha / wohi tugun*
He eats rice	*git kadi gaman*
You eat rice	*nu kadi naman*
(You) come here	*taba aaga*
(I) drink water	*tang chongdi*

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[11] One informant (Rajamama) says *chi kadi gaman,* while another (Puni Thakuri) says *kadi tamdi.*

[12] *woha* 'house, home' in the MW dialect, *wohi* in the C dialect.

I go along this way	*taun chahan*
I live at home	*wohi sahan*
I live in(side) the home	*aawa sahan*
It rained	*tang ugun (tãun)*
It rained yesterday	*pene tang ugun*
The sun rose	*ing ugi*
A hard sun!	*ing haap!*

Comparative Study of Kusunda with the Magar Language of the Karnali Area[13]

The Kusunda and the Magar languages of the Karnali area have distinct similarities in common. However, we can find a number of Kusunda words that are similar to words in other Tibeto-Burman languages like Shauka, Baram, Chepang, Tamang, Thaksya, Bhote, Bhujel etc. The Magars of the Karnali area call the languages of others (including Nepali, the state language of Nepal) *rangpang* and call their own language *gepang*. Kusundas call their own language *gipan/gepan* ('tongue'). Both *gepang*'s have striking syntactic similarities in common:

Kusunda *gipan*	**Western Magar** *gepang*
chi kadi gaman 'I rice eat'	*nga yai/kang jyonga* 'I rice eat'
git kadi gaman 'he rice eats'	*wola yai/kang jyowa* 'he rice eats'
nu kadi naman 'you rice eat'	*nanga yai/kang jyona* 'you rice eat'

In the Kusunda sentences given above there are pronominalized verbal prefixes, and in Western Magar sentences we can see pronominalized verbal suffixes. Have prefixes and suffixes different functions in the process of word formation? Below I give a few more pronominalized sentences and phrases from the Magar language of the Karnali area that resemble the Kusunda structure:

Western Magar *gepang*

nga ri ngawoje	'I water drink'
nang ri nawoje	'you water drink'
ge ri gewoje	'we water drink'
nga ngalijjya	'I (here) am'
nang nalijjya	'you (there) are'
ge gelijjya	'we (here) are'
nga ngado	'I (it) do'
nang nado	'you (it) do'
ge gedo	'we (it) do'

The above examples indicate that Kusunda is not a language isolate. It is quite similar to the Kham Magar language of the Karnali area, which shows that Kusunda has some sort of affinity with Tibeto-Burman. Unfortunately, Hodgson-Grierson and Reinhard-Toba do not seem to have made any effort to compare Kusunda with other Tibeto-Burman languages found in Nepal. Had they been able to study the Western Magar language, they would have categorized Kusunda as having originated in the Sino-Tibetan area.

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[13] Known as the Kham Magar language.

Comparison with Kulung Rai Language

The eastern part of Nepal is primarily inhabited by the Rai and Limbu ethnic groups. These peoples speak their own languages belonging to the Tibeto-Burman family. Kulung Rai is also a pronominalized language. Below I give a few words and sentences of Kulung Rai language:

Kulung Rai words:

kong 'I'	*pu* 'snake'	*waas* 'play'
nako 'he'	*namchhoba* 'sun'	*lung* 'stone'
keika 'we'	*wadi* 'egg'	*lung* 'leg'
kaska 'we two'	*kaw* 'water'	*seechho* 'tree'

Kulung Rai counting:

ibum 'one'	*ilpo* 'one person'
nichi 'two'	*nippo* 'two persons'
supchi 'three'	*suppo* 'three persons'
lichi 'four'	*lippo* 'four persons'
ngachi 'five'	*ngapo* 'five persons'

Kulung Rai sentences:

'I eat rice'	*konga ja choyo*
'he eats rice'	*nskosa ja choyo*
'we eat rice'	*keika ja chyaeka*
'we (two) eat rice'	*kaska nippo ja chaichuka*
'we (two) play'	*kaska was laichika*
'we (many) play'	*keika lai laiyaka*

Some linguists have attempted to categorize Tibeto-Burman languages spoken in the Himalayan belt as the "Tibeto-Himalayan" sub-branch of Sino-Tibetan, which consists of pronominalized Himalayan languages belonging to the "Other Mongoloid" and non-pronominalized Himalayan Languages belonging to the "Kirats" (Risely et al. 1931/1975). If we follow them Kusunda falls in the "Other Mongoloid" pronominalized language group. But this interpretation has not been acclaimed by all. The Santhal language belongs to the Austro-Asiatic Munda family. It also has pronominalization, but this is different from that of the Kusunda language.

General Conclusion

One of the most powerful functions of a language is that of a repository of the culture and worldview of its speakers. Its grammar and lexicon store the shared experiences of past generations, and a language is the channel by which these images, emotions, knowledge and beliefs are transmitted to the next. A language does not just transmit messages; it decorates them aesthetically, and so facilitates their reception and retention.

In the preceding paragraphs I have explained how important the Kusunda language is for serious studies. Kusunda appears to have external relationships with a number of indigenous peoples' languages spoken across the world – from Alaska of North America to South and Central Africa, Europe, Asia and New Zealand. (See Appendix 2).

I believe Kusunda, as one of the ancient languages, could also be a reliable tool for understanding the prehistory of early peoples in South Asia. It is therefore high time that the Kusunda language be preserved in an equitable manner. The Kusundas should not be deprived of their human rights. Following the declaration of the rights of persons belonging to national, ethnic, religious and linguistic minorities[14] His Majesty's Government of Nepal has been very keen to implement an integrated community development program for the indigenous peoples, including the Kusundas, who are living in various parts of the country.

Since the Kusundas are in a state of impoverishment, they urgently require genuine support from among governmental as well as non-governmental organizations that are working for the all-round development of the peoples of Nepal. By bringing the Kusundas together in one place and encouraging them to communicate among themselves in their own language, the Kusunda language can be stabilized. There are some other Kusundas, of mixed origin, who also wish to learn this language and seek our support. Additionally, in order to preserve other Himalayan languages we should undertake a further Linguistic Survey of Nepal, employing professionals (especially from among those of the speakers' own communities), so that we might be able fully to understand Kusunda and other languages in the Himalayas.

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Appendix 1: Kusunda Vocabulary[15]

Old data	**New data**	**English equivalent**
nabi	*amokh* (MWN)	hand
uyu (CN)	*lapa* (MWN)	blood
gihan	*myau*	female sex organ
gibhu	*konji*	male sex organ
gwa	*gwa*	egg
-	*amba* (MWN)	meat
nyu	*nyu*	man/person
manenu	*manenu* (MWN)	many people
-	*kugjangnu* (MWN)	few people
-	*bai* (MWN)	sister
ngyangdi	*nangdighichi* (MWN)	woman

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[14] Adopted by the United Nations General Assembly: Resolution 47/135 of 18 December 1992.

[15] Some of the words in the list have been recorded quite recently. Kusunda seems to have eastern (CN) and western (MWN) dialects. For example: 'blood' in Kusunda is *uyu*, and 'monkey' is *guinyau*. But recent data differ, as my informants say *lapa* and *haku* respectively. **NB**: CN= Central Nepal, MWN = Midwest Nepal, HG = Hodgson and Grierson, RT = Reinhard and Toba.

Old data	**New data**	**English equivalent**
dhukchi	*duighihi*	son
makche	*makche*	young man
oichindi	*usindi* (MWN)	young woman
-	*dhaiya* (MWN)	old man
ghichi	*gheche* (MWN)	child
gipan	*gipan/gepan*	language
guinyau (CN)	*haku* (MWN)	monkey
nikhumba	*nongba* (MWN)	ox/cow
miza	*ajaki* (MWN), *miza* (CN)	goat
tapghichi	*tapghichi*	chicken
-	*tapgimi*	cock
ii	*ii*	tree
-	*syangwa*	large tree
-	*gelang*	forest
aayi	*pai* (CN)	bamboo
gipan	*gipan*	flower
gitak	*gitak*	seed
-	*hyo*	mango seed
itak	*itak*	root
	ipan	maize
-	*sising* (MWN)	paddy
-	*kadida* (CN)	rice (uncooked)
kaadi	*kaadi*	rice (cooked)
-	*paiti* (CN)	pulses (legumes)
-	*abokh* (CN)	yam
-	*abo*	vegetables
-	*abu*	yam
mangmi	*mangmi*	bread
dintakanyia	*tang*	wine
-	*dimtang* (CN/MWN), *sijang* (MWN)	beer
jing	*jing*	mustard oil
wou	*wou*	stone
gali	*gali*	sand
-	*huki*	salt
tang	*tang*	water
za	*za*	fire
jai	*jai*	ashes
kai	*kai*	wind

Old data	**New data**	**English equivalent**
bokh	*garhu* (MWN)	warm
-	*puhut* (MWN)	hot
-	*yakkau* (MWN)	cool
khangu	*khangu* (MWN)	cold
ohin	*ohin*	nice[16]
sara	*sara*	long
tut	*tut*	bow
-	*mui*	arrowhead
-	*phuchi*	stool
aicha	*archa* (MWN)	needle
-	*suta* (CN/MWN)	thread
-	*bukta*	clothes
-	*gigzi* (CN), *dazzi* (MWN)	firewood
-	*pungar* (CN)	haystack
un	*un*	trail
wohi	*woha*	house
lahang	*langhai*	village

Words for family relations:

Old data	**New data**	**English equivalent**
mai	*mai*	mother
yehi	*yehi*	father
-	*bai* (MWN)	sister
-	*bhaya* (MWN)	younger brother
-	*nyakham* (MWN)	maternal uncle
-	*nangbi* (MWN)	maternal aunt
-	*yangzar* (MWN)	great uncle
-	*mizarni* (MWN)	great aunt
-	*yamala* (MWN)	middle uncle
-	*yamali* (MWN)	middle aunt
-	*yaisala* (MWN)	younger uncle
-	*maisali* (MWN)	younger aunt
-	*yakanchha* (MWN)	youngest uncle
-	*makanchhi* (MWN)	youngest aunt

Parts of the Body:

ipi	'head'
ipi	'hair' (CN)

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[16] 'beautiful, pleasant, gentle'

gee	‘hair’ (MWN)
ining	‘eye’
inau	‘nose’
aata	‘mouth’
nabi	‘arm’
amokh	‘hand’
hanki	‘neck’
yan/yang	‘foot’
okchi	‘chest’
ambu	‘breast’
idu	‘liver’
gepo	‘flesh’
gu	‘bone’
konji	‘male sex organ’
myau	‘vagina’ (CN)
uyu	‘blood’
imat	‘stomach’

Pronouns:

chi	‘I’	*nu*	‘you (thou)’	*git/gina*	‘he, she, it’
chiyi	‘my’ (HG)	*niyi*	‘thy’ (HG)	*gida*	‘he, she, it’ (HG)

Verbs:

tagai	‘go’	*chaanaan*	‘(I) go’	*dagai*	‘went’	*aaga*	‘come’
taman	‘(I) eat’	*itanan*	‘give’	*kamaji*	‘bite’		
aganan	‘(he) makes’	*nyawan*	‘to collect’ (RT)				

Names of mammals, birds, reptiles, insects, etc.:

mayhaq	‘tiger’	*kauli*	‘tiger’
aagai	‘dog’	*mahi*	‘water buffalo’
guinyau	‘monkey’ (CN)	*haku*	’monkey’ (MWN)
samtak	‘squirrel’	*nikhumba*	‘ox/cow’ (CN)
nongba	‘ox/cow’ (MWN)	*amba*	‘an animal living in trees’[17]
yangut	‘mongoose’ (CN)	*tap*	‘jungle fowl’
miaa	‘lion’ (?) (RT)	*tu*	‘snake’
kee	‘louse’	*pui*	‘a kind of amphibian’

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[17] A kind of primate.

Appendix 2: The External Relationships of Kusunda with other Languages[18]

1. 'man'

KUSUNDA : _ñu ~ niyu_ 'man'; Ainu _ainu, niyu_ (person); INDO-PACIFIC : [Timor-Alor-Pantar] Makasai _anu_, Bunak _en_ 'man'; NA-DENE: Tlingit _na_ 'tribe, people'; SUMERIAN : _na_ 'person', _ni-ta, ni-tah_ 'man'; INDO-EUROPEAN : *_ner_ 'man, male'; ALTAIC *_niarV_ 'man, person'; KADU : Miri _ñuri_; NIGER-CONGO : Mande _nu_, Kpelle _ñu_ 'man', Bambara _nyi_, Nalu _nyie_, Mossi _ni_, Kasele _onyi_, Adele _(e)ni_, Yoruba _ni_, Likpe _nii_, etc.

2. 'belly'

KUSUNDA : _imat, tamat_ 'belly'; AMERIND *_mat;_ TASMANIAN [W, SE] _lomati(na)_; NA-DENE: Athabaskan *_wət_ 'belly', Eyak _wət_ 'vomit'; BURUSHASKI -_wat_ 'body, self'

3. 'egg'

KUSUNDA : _goa, gwa_ 'egg'; ANDAMAN: Onge _gwagane_ 'turtle egg'; SINO-TIBETAN: *_Qo(w)H_ 'egg' > Tibetan _s-go-ŋa_ 'egg(s)', etc.; NA-DENE: Haida _qaw_ 'bird egg'

4. 'water'

KUSUNDA : _tang_ 'water'; NA-DENE : Haida _taŋ_ 'sea water', Eyak _tãh_ 'waves', Galice _ta-_ 'water' (in compounds), Chipewyan _tà-_, Navajo _tá-_ 'water'

5. 'fire'

KUSUNDA : _dza, za_ 'fire'; Sino-Tibetan *_tsha_ 'hot'; SUMERIAN : _i-zi_ 'fire, ALTAIC *_asa_ 'to ignite'; Gilyak _t'a_; KHOISAN: Hiechware _joaa_; INDO-PACIFIC: Moni _usa_ 'fire'; DENE-CAUCASIC : Basque _su_, Caucasic *_ts'ayi_ 'fire'; NILO-SAHARAN: Lendu _kazz_ 'fire'; NIGER CONGO : Bambara, Dyula, Mano, Vai _ta._

6. 'house'

KUSUNDA : _wohi_ 'house'; INDO-PACIFIC [SNG] Hiwi _aba_, Sesa _pei_ 'village', Jelmek _ebi_; NIGER-CONGO: Tschi _o-fi; o-fi-é_ 'home', Nyangbo _ke-pi_ 'home, house', Bamana _pu-γe_

7. 'thunder, lightning'

KUSUNDA: _khila_; ANDAMAN: Bale _kuru:dʌ-ke_ 'thunder'; AUSTRIC: Indonesian *_kilat_ 'lightning', *_kilap_ 'glitter'

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[18] These are few of the selected examples drawn from Paul Whitehouse's work showing the external relationships of Kusunda language and its importance for studying prehistory of the South Asian peoples. I have drawn them from the writer's article in _Mother Tongue_ (1997).

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More on Basque vis-à-vis Eurasiatic

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In an earlier article in *Mother Tongue*, "Basque Parallels to Greenberg's Grammatical Evidence for Eurasiatic" (Thornton 2002), I identified certain Basque grammatical formatives as similar or even parallel to some of Greenberg's Eurasiatic grammatical formatives. Over half of the comparisons related to Basque and Japanese in particular, and a probability of homologous developments in some cases, given the great geographical distance and time depth involved, was understood; thus the a close Basque-Japanese relationship was obviously rejected. It seemed evident that the almost uncanny similarities between the Basque morphs *ko* and *te* and their apparent Japanese counterparts could perhaps only be explained by appealing to what Larry Trask recognizes as the "astonishingly conservative" nature of Basque (Trask 1997: 47) and what I take to be the deeply conservative nature of Japanese as well.

Certain unfortunate errors appeared in that essay, however--errors for which I am wholly responsible; in the present paper I correct these, and in the process present additional evidence in support of my original thesis. I am grateful to Professor José Ignacio Hualde for calling several of these to my attention and providing important related insights into Basque (personal communication, 2003). Four grammatical formatives are involved. They are identified below by their original numbering in the 2002 article. "Diminutive K" (1.1) is addressed last of all. Basque items are cited in present-day standard Basque orthography.

1.2 (Passive) Participle T (Additional discussion)

In this, as in my earlier article, I raise the possibility of a genetic connection between Greenberg's Eurasiatic grammatical formative "Participle T" the Basque and Japanese progressive constructions, which are formed on the Basque and Japanese morphs *-te* respectively. In the comparison which follows, the roles of Basque *-n* and Japanese *-n* are examined and in Basque and the full form of the Japanese progressive construction is given, both for the first time. Basque (*te* may alternate with *tze*, a palatalized variation):

Euskara ikas- t(z)e- n ari da.
Basque-language study *-te* LOC EXIST be-3sg
'He/she is studying/learning Basque'.

Japanese:

Basuku-go- benkyō-o si- te iru -n da.
Basque-language- study ACC do *-te* EXIST GEN COP
'I am studying Basque; you, he/she/we . . . (etc.).

Note that not only is Japanese (postposition) *-n* a genitive rather than the typologically expected locative in the progressive, but also that its position vis-à-vis the Basque progressive is transposed (or vice-versa): whereas Basque locative *n* suffixes to *te*, in Japanese existential *iru* intervenes between *te* and genitive *n* (full form *no*). Also of importance is that in Old Japanese (prior to the eighth century) the existential verb in the progressive construction was not *iru* but *ari*; that is, the existential verb was identical in form to Basque *ari*, e.g.: *Mi-te ari da* (*mi-* 'see, look, watch'). The *no . . . da* augmentation evidently represents a more recent development. Evidently the Basque and Japanese *te*-progressives represent homologous or independent developments, but whether Japanese *te* truly goes back to a common ancestor with Basque *te*, the former, at least, presumably having arisen out of the inherited inner resources of the Japanese ancestral language, is an assumption surely open to challenge, and more research is surely called for.

1.10 Negative M (Correction of error)

In my 2002 paper I suggested, unfortunately, that the *m* of Basque *damurik* might be linked to Greenberg's (prohibitive) formative Negative M (119). Hualde points out (personal communication, 19 December 2002) that the Basque word is a borrowing from Latin *damnum*. A simple check in a Basque dictionary would have revealed the extreme paucity of native Basque words in initial *d-*.

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And in another hurried error, the correct meaning of the Basque doublets *hegal* and *magal* is 'wing' not 'pear' (121). I apologize for these lapses.

2.1 Negative S (Correction of error)

The Basque negative particle *ez* 'no, not' and the Japanese negative verbal suffix *-zu* (which in Old Japanese alternated with *-nu*) were compared and a possible correspondence suggested simply on the basis of a surface resemblance, an unpardonable violation of responsible scholarship. I now have learned that Japanese linguists are "in basic agreement" that Japanese *-zu* (now archaic) arose from a syncopation of the infinitive *-ni* + *su* 'do'" (J. Marshal Unger 2000: 674). However, Unger notes that "the cause of this innovation has never been satisfactorily explained." Nevertheless, "it seems certain that the negative morpheme started off as a closed root in *n*. The coronal lenition rule suggests a structural reason" (Unger 2000: 674).

Diminutive K (Correction of error; additional discussion)

The original impetus for my 2002 essay was R. L. Trask's examination of the Indo-Europeanist Antonio Tovar's claim that the similarities between the Basque suffix *-ko* and Proto-Indo-European **ko* must go back to "some common source" by virtue of Pre-Basque and Proto-Indo-European having belonged to "an ancient European linguistic area," where they had some "'proto-historical relationship'." Tovar postulated, in Trask's words, "'a puzzling construct'" that was "something less than a genetic relationship but something more than mere contact, and apparently even something more than a *Sprachbund*" (Trask 373-376).

In that paper I attempted to establish a link between Trask's Basque "relational (or adnominal)" suffix *-ko* and what I termed the Japanese "locational" suffix *-ko*. (As noted there, Japanese *-ko* is suffixed to the interrogative stem *do-* 'wh-?' to derive the interrogative *doko* 'where?', and to three deictic stems: proximal ko-, mesial so-, and distal aso-, to derive four pronouns with locative properties). Unfortunately, I juxtaposed Basque *Non dago*? 'Where is X?' with Japanese *Doko da?* 'Where am/is/are X?' to illustrate the link. Contrary to the above, the *-go* of Basque *dago* is not *-ko*; rather, *-go* is an integral part of *dago*, the third-person singular of the verb *egon* 'to stay, to be' (cf.

Spanish *estar*). (The full present-tense conjugation is: *nago*, *zaude*, *dago*; *gaude*, *zaudete*, *daude*.) I am very sorry for the error.

This is not to refute my original contention that locative properties reside in Basque relational *-ko* and Japanese "locational" *-ko* in such a way as to suggest a similar "affinity" for the locative. As Trask states, "the addition of Basque *-ko* to an NP in the *locative* is particularly frequent" (emphasis added). Among the examples he provides to demonstrate that relational *-ko* is not a case ending, whether locative or genitive, are genitive constructions such as *etxearen kolorea* 'the color of the house' and *etxearen historia* 'the history of the house' contrasted with *-ko* phrases such as *etxeko andrea* 'the mistress of the house' and *etxeko gelak* 'the rooms in the house'. "The *-ko* phrase", he clarifies, "is used to mark something which is *physically present* in the house or which forms *a physical part* of the house or a person who lives in the house" (emphasis added). Thus:

> This frequent addition of *-ko* to an NP in the locative, combined with the loss of the locative-case ending before *-ko*, is what led many earlier investigators into concluding (wrongly) that Basque had a distinctive 'locative genitive' case in *-ko* [e.g. *etxeko andrea* 'the mistress of the house'], a special genitive case expressing a relation of location. It does not. (Trask: 102)

Like Basque relational *-ko*, Japanese "locational" *-ko* operates as an adnominal suffix, but not, on the other hand, as a "relational" suffix--unlike the uniquely Basque *-ko*, in that the Japanese suffix cannot directly precede a nominal (the postposition *no* 'of' must intervene in order for a relationship between the two nominals to be signaled).

An additional similarity, though, is that Japanese *-ko* and Basque *-ko* both lack semantic content. By contrast, lack of semantic content does not obtain for either Trask's Basque "derivational" *-ko* (Trask 373 f.), which derives mostly diminutives (and a smaller number of augmentatives), or the Japanese derivational prefix *ko-*, which derives diminutives (only). Like modern Japanese diminutive *ko-*, the basic meaning of Japanese locational *-ko* would have had to be "small"; and, as suggested in the earlier paper, perhaps one can make a case that Japanese suffixed *-ko* must have acquired its "locational" function by means of a semantic association of "location" with a narrowing down to or a focusing in on a delimited area. One can argue that a similar semantic process may well have taken place in Basque as well, leading to the evolution of the morph *-ko* into the unique relational element we see in historically-attested Basque today. It does not seem entirely inconceivable that the notion of "small" led to a relational function through, again, a semantic process of connecting the idea of "small" with a focusing-in on a grammatical relationship between or among separate entities.

If one is to build a case for a genetic link, one must, however, deal with the fact that in Basque diminutive *-ko* is a suffix whereas in Japanese it is a prefix. This may or may not be a serious obstacle. Basque is almost exclusively suffixing, and the few exceptions, Trask reports, seem to be recent innovations from outside. As for Japanese, the fact that it is an agglutinative language (whereas Basque has fusional characteristics) appears to have a bearing on its toleration of prefixing, Shibatani Masayoshi seems to suggest (Shibatani 1990: 217). And "[w]hile there are both nominal and verbal suffixes, most prefixes are affixed to nouns." However, other than diminutive *-ko*, it would seem, prefixing appears to be highly restricted to a limited set of nouns and adjectives and to a few prefixes of native provenance; moreover, Shibatani observes, "Sino-Japanese affixes are more productive than the native Japanese affixes. From the historical point of view, productive affixes are those newly introduced, whereas the non-productive ones are remnants of old native forms" (218). It would appear, therefore, that Proto-Japanese must have been predominately a suffixing language, but it is not clear to me whether *ko* as a deriver of diminutives was always prefixed.

The loss of the locative-case ending before *-ko* is intriguing, and, as suggested by the comparison just above, seems to call for further research. For can we not legitimately question whether this loss of locative *-n* has something to do, perhaps very remotely, with the later development in Indo-European of the Germanic and Slavic adjectival endings built on *-ko*, involves the prefixing of **-i* and **-s* to produce **-isko* (see my discussion of Basque *-zko* and Proto-Indo-European **-isko* in my previous essay: 115-116)? I think so.

The operation of Trask's "relational (or adnominal)" *-ko* defines its "canonical use" (Trask: 375). Trask identifies problems one encounters in attempting to establish a genetic relationship between relational *-ko* and Proto-Indo-European **-ko* (Trask: 373-375), and concludes that, in several respects, the one "does not look much like" the other

(375). Addressing all the facets of the argument for a genetic relationship with PIE, however, Trask reviews the other functions of *-ko* as well. First, as he shows, *-ko* can be added to certain types (only) of N-bar "to derive an adjectival modifier which, like the more usual type of *-ko* phrase, precedes its head." The conditions of its realization are extremely limited, in that in Basque an adjectival modifier follows the NP it modifies. One of the examples provided by Trask is the following:

bihotz oneko neska
heart good-*ko* girl-DET
'a good-hearted girl' (375)

As with Trask's comparison of Basque relational *-ko* and Proto-Indo-European **ko*, the fact that Basque derivational *-ko* is a suffix whereas Japanese derivational *ko-* is a prefix presents a syntactic problem in any attempt to argue for a possible genetic connection. However, it is interesting, if nothing else, to note that Japanese derivational *ko-*, which, as we have said, derives diminutives of nouns, can also be prefixed to certain adjectives (which, in Japanese, precede the noun): adjectives derived either by suffixing *-na* to a bound adjectival stem or to ancient (true) adjectives, in *-i*, to form derivative adjectives, e.g. *kogirei na* 'neat, tidy' (*kirei-* 'clean, beautiful') and *kozatoi* 'a little clever (derogatory)' (*satoi* 'clever, intelligent') respectively.

As a nominal prefix Japanese derivational *ko-* functions as an adjectival, according to Morita Yoshiko, e.g. *ko-inu* 'small dog', i.e. 'puppy', *ko-isi* 'small stone', i.e. 'pebble' (Morita 2003: 11). "However", she points out, "when appearing with a particular kind of idiomatic predicate, *ko-* attached to a noun can function as a manner adverb, whose meaning is 'a little' or 'lightly'." In this construction, *ko-* "modifies its predicate as a whole rather than its nominal head," e.g.:

[*ko-gosi*] *-o kagameru*
[little waist]-ACC stoop
'to stoop slightly at the waist' (Morita: 11)

Morita points out that constructions of this type are all idiomatic and appear as rather fixed phrases in which "the nominal head accompanied by *ko-* expresses a part of the body"; and it appears that *ko-* may be attached to a few other nouns as well to produce verbal derivatives which express the idea of physical sensation or suggest some personal relationship to a physical entity, so long as the noun refers to something related to one's physical existence. One can say, for example, *ko-gane -o tameru* 'save a little money' (*kane* 'money'). Relevant to this particular construction she notes also that "[t]here are other prefixes in Japanese that appear to behave just like *ko-* in [this] context," and that these also "attach to body part nominals in idiomatic phrases," e.g. *usu-me-o akeru* 'to open one's eyes [s] lightly' (*usu-* 'thin, light', *me* 'eye') (after Morita: 12).

Let us now return to the Basque derivational suffix *-ko* and compare the above Japanese idiomatic construction with another of its functions. In addition to deriving diminutives of nouns (as well as, rarely, augmentatives), as was mentioned earlier, Basque derivational *-ko* can be added to a noun or to a numeral to produce a derivative that is also a noun, according to Trask (375). He states:

> This cannot be done freely, and indeed the word-forming suffix is unproductive or only weakly productive. The meaning of the derivatives are generally unpredictable, though those formed from body parts often denote either a blow to the appropriate part of the anatomy or clothing or jewellery for that part. Examples include *gerriko* 'girdle' (*gerri* 'waist'), *zortziko* (a particular dance for eight people) (*zortzi* 'eight'), *marmitako* 'stew' (*marmita* 'stewpot'), *ipurdiko* 'smack on the arse' (*ipurdi* 'buttocks') and *belarritako* 'earring' (*bellari* 'ear'). (pp. 375-376)

The prefixing as opposed to the suffixing of *ko* in Japanese and the presence, in contrast to the absence, of a predicate places the Basque and Japanese constructions at odds with one another syntactically, weakening the case for a genetic link. Yet, of course, there is a similarity in that in both languages constructions with *ko* naming or referring to the limited sphere of the human body or to entities or phenomena that touch upon one's existence or well-being in some manner. Perhaps one can counter that we are dealing with a typological phenomenon. But how to explain the presence of *ko* in both cases? Syntactically, however, here is another instance of mirror opposites; and in the Basque, moreover, the original semantic content of the suffix would seem to be weakened or compromised. Nonetheless it seems unwise to dismiss summarily the odd similarities inherent in the Basque and Japanese constructions.

1. It is not a derivational suffix, but a fossilized syntactic element which can be added to one interrogative and three deictic stems only;
2. It attaches to four adverbial stems.
3. It derives four locative pronouns, not adjectival modifiers; thus it does not perform a relational function.
4. It has no semantic content.

There is an enormous quantitative gap in the productivity of Basque *-ko* and non-productivity of Japanese *-ko*, and the latter seems to have become non-productive at a very early stage, if indeed it ever was productive at all, but the two seem to share some kind of functional property on a primitive level. In my view Basque relational *-ko* and Japanese "locational," *-ko* are to an extent comparable grammatical formatives, and as such seem to lend support to the possibility of a deep kinship.

Trask's comparison of PIE **-ko* and Basque relational *-ko* reveals significant differences, and he therefore finds no convincing evidence for regarding Tovar's proposal of an extremely close historical, even though not genetic, relationship as anything more than "at best an implausible conjecture, at least until someone turns up more extensive evidence for an ancient *Sprachbund* involving Proto-Basque and PIE" (376). In this paper I have offered a comparison of Basque *-ko* not to PIE **-ko* but rather to Japanese *ko*, an approach precipitated by Greenberg's evidence; and on these grounds I argue that we must consider probable deep kinship between Basque and Eurasiatic.

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In sum, can we legitimately compare Basque *-ko* with Japanese *ko*? In the manner of Trask's summary of the properties of Proto-Indo-European **-ko* and of Basque relational *-ko* as a test of Tovar's claim of their relatedness (374-375), let us list the properties first of Basque derivational *-ko* and Japanese derivational *-ko*, and then of Basque relational *-ko* and Japanese "locational" *-ko*.

First, Basque derivational *ko-*, as I understand it:

1. It is a derivational suffix.
2. It is affixed to nominals.
3. It derives diminutives and, more rarely, augmentatives.
4. It would appear to retain semantic content, even if weakened by ambiguity.

Now, Japanese derivational *-ko*, also as I understand it:

1. It is a derivational prefix.
2. It is affixed to nominals (and to some adjectivals).
3. It derives diminutives.
4. It retains semantic content.

The main difference would appear to be that Japanese derivational or diminutive *ko-* is prefixed whereas Basque derivational *-ko* is suffixed; but, in my opinion, neither this or the other differences seem insurmountable from a developmental or typological point of view. I regard Basque derivational *-ko* and Japanese derivational *-ko* as probably being comparable as grammatical formatives.

Next, a comparison of the main functions of Basque relational *-ko* and Japanese "locational" *-ko*. First, the Basque suffix; these are its three main or "canonical" functions:

1. "It is not a derivational suffix, but a syntactic element which can be added freely to any constituent of an appropriate type."
2. "It is attached to adverbials, regardless of their internal structure."
3. "It derives adjectival modifiers which behave quite differently from lexical adjectives."
4. "It has no semantic content." (Trask: 375)

Now, the Japanese suffix (closely paraphrasing Trask):

Unger, J. Marshall. 2000. "Reconciling Comparative and Internal Reconstruction: The Case of Old Japanese /ti, ri, ni/." *Language,* vol. 76, no. 3 (September 2000), 655-681.

Note from an interested by-stander

Professor Thornton's fine paper takes no position with respect to the **Borean** hypothesis, of course. In relating grammatical elements in Basque to their counterparts in Japanese he has presented evidence linking Basque to the phylum to which Japanese belongs under the Greenberg Eurasiatic hypothesis. Thornton makes that clear; it is his intent. Moreover, by doing that coupling Thornton has potentially presented grammatical evidence supporting the Borean hypothesis; that was quite possibly **not** his intention. However, since Basque represents one of the principal components of the Borean structure, **Vasco-Dene** or Dene-Caucasic + Basque + Burushaski, any connection with Japanese which represents another principal component, **Eurasiatic**, has the effect of supporting the Borean hypothesis. Now of course Basque may not actually be a bona fide member of Vasco-Dene, while Japanese which is possibly more fought over than even Basque may not be a bona fide member of Eurasiatic. **Paul Benedict** has not yet been decisively defeated in proposing that Japanese is in fact a member of **Austro-Thai**..

When more formal collations of grammatical evidence for Borean are at hand, especially between the extremes in Afroasiatic and Amerind, we will find that connections such as those proposed by Professor Thornton are most valuable. Moreover, a large number of etymologies proposed for Nostratic (before the Eurasiatic excision) involved Afroasiatic. For example, the Basque and Japanese link through a passive in [-te] is very likely to be cognate with [-t-] passive found widely in Afroasiatic, joining such distant kindred languages as Jibbali of Modern South Arabian and some Nomotic languages in southwestern Ethiopia

A Note on “Tamil and Japanese”

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In addition to the more-or-less accepted Altaic and Austronesian strata, Susumu Ono (2001) has added a third element in the genesis of Japanese, *viz.* Tamil (Dravidian), which might be seen (though not by Ono) as evidence for a pre-Harappa/Jomon connection. However, before postulating such an input, one should investigate whether this stratum might not be explained along the Altaic or Austronesian line.

To exemplify, the “element function” (Louis Hjelmslev) Tamil *pp, p* ~ Japanese **p > f* should find support, among other cases, in:

(1) Tamil *pat-u* ‘to perish, die’ ~ Japanese *fatu* (Ono 2001: 121).

However, as already pointed out by the Edward Sapir disciple Benedict (1990: 180-181) for his reconstruction of Proto-Austro-Tai (PAT) **(ma-)play* ~ **pa-play* ‘die/end ~ kill’, the Proto-Austronesian reflex is **maCəy* = **maCay* ‘die’ ~ **paCəy* = **paCay* ‘kill’, and it is also reflected in Japanese *Fate* ‘end’ (noun) (< **pa-*; where *F* is a bilabial fricative).

(2) In the same way, Tamil *pal* ‘tooth’ ~ Japanese *fa* (Ono 2001: 126) can be dealt with: cf. PAT **(N)Gi(m)pan* ‘tooth’ > Proto-Miao-Yao **p[aay]* ‘molar tooth’, and > Japanese *Fa* (Benedict 1990: 255).[1]

(3) Also, an example from the intervocalic position: Tamil *cūpp-u* ‘to suck’ ~ Japanese *suf-u* (Ono 2001: 124) might better be seen within the PAT framework: PAT **(ň)tšuptšup* ‘suck’ > Proto-Tai **suup* > Siamese (Thai) *suup* ‘suck in with the mouth’, etc., and > Old Japanese *suF-i* ‘suck, sip, inhale’ > Japanese *su-i* (Benedict 1990: 250).

Thus, it seems possible to re-analyze these Tamil-Japanese “cognates” as Japanese reflexes of PAT; in contrast to Ono, Benedict thinks of an Austro-Tai origin of the Yayoi people. Naturally, an Austro-Tai stratum in Dravidian may also be postulated (cf., *e.g.*, Ohnishi 1999: 202) to account for the Tamil-Japanese correspondences.

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[1] Cf. the Altaic alternative: Old Japanese *pa* ‘tooth’ < Proto-Japanese **pà,* id., connected by Ramstedt, Susumu Ono and others with Korean **par* ‘tooth’ (Middle Korean *ni-s-par*), and further with Tungusic **palV* ‘molar tooth’ (S.A. Starostin, *Altajskaja problema i proiskoždenie japonskogo jazyka,* Moscow: Nauka 1991, p. 109, no. 8). Some Nostraticists trace this Altaic **palV* ‘tooth’ and Dravidian **pal* ‘tooth’ to a common Nostratic root. [Ed.]

Eskimo Materialism: *Inuk* 'Person' and Other Original Duals

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In his "Reflections" on Greenberg (2000), Bomhard (2002: 93) also invokes the dual KI(N) – well attested in Eskimo *–k* (*e.g., nunak,* the dual of *nuna* 'land', etc.; in Aleut cf. *tanax* ~ *tana-* id.).

The dual – which might be regarded as confusing and superfluous – plays a central part in Eskimo morphology, even having influenced the singular/plural endings – and not *vice versa* – which therefore also might help answer Bomhard's question about "a Proto-Eurasiatic plural marker **-kV* ... preserved in Armenian."

A dualistic world-view is in fact not that bad, as it seems to correspond to human experience: left and right, north and south, up and down, long and short, black and white, good and evil, warm and cold, young and old, man and animal, husband and wife, father and son, life and death, heaven and earth, etc. Consequently, a natural attempt to order these concepts seems to be to do it in pairs, and that is what the Eskimo language has carried out. And one discovers in addition that the dual is not so much the number "two" but a pair, it is not "1 + 1" but rather "something belonging together, connected, a pair."

The Eskimo noun ends in most cases in a vowel or in the uvular stop *–q* (belonging to the root or marking the singular); there is a little group of words ending in *–n.* And there are not a few words ending in *–k,* which can reflect earlier *-ŋ,* but which in most cases is original, *i.e.,* really is the old dual ending (whereas *–t* marks the plural).

To illustrate, here are some examples from my Greenlandic notebook.

I. Two of the same kind:
   1. Double-existent: *qutuk* 'collar-bone', *kiasik* 'shoulder-blade', *mulik* 'nipple', *ikusik* 'elbow', *kingmik* 'heel', *suluk* 'a bird's wing', *kamik* 'boot'
   2. Twofold: *nigsik* 'hook', *qatik* 'a bird's chest-bone', *mumik* 'book-sheet', *kinak* 'face', *qiak* 'skin'
   3. With two end-points: *tukik* 'length', *nukik* 'sinew', *kuuk* 'river, creek', etc.

II. Two not of the same kind:
   1. A base and a top: *nuuk* 'cape', *ingik* 'mountain-top', *agssak* 'finger'
   2. Belonging together: genitalia (male and female), bow and arrow, egg-shell and yolk, husband and wife, mother and daughter
   3. The half of two unequal parts: *ilik* 'helper' (presupposing someone receiving), *sak* 'front' (presupposing 'back'), etc.

Even the best-known Eskimo word has the *–k*-ending: Greenlandic *inuk,* Yupik Eskimo *yuk* 'person' (< **ənyuk:* where in Greenlandic *i* < **ə,* and in Yupik loss of **ə* and change of the palatal nasal > *y*). Is it possible here to discover anything dualistic?

In the history of Materialism, the two "abstract" principles of form and matter (cf. Latin *māter*) have played a dominant role, reflecting the two *Urprinzipien* of Male (creating) and Female (receiving). Maybe this myth is represented in Eskimo *inuk* (originally 'male-female =) person'. Compare also the Na-Dene lexical parallel (no. 176 in Blažek & Bengtson 1995, pp. 36, 163). A compound meaning 'female-male' is still analyzable in Finno-Ugric, *e.g.* Udmurt

(Votyak) *nyl-pi* 'child' (= 'girl-boy'), or in Ainu *ni-kob-un* 'Gilyak' (= 'woman-man', *fide* Bouda 1960: 383).

Eskimo materialism as exemplified in its dualistic world-view is naturally different from philosophical Dualism as found in Plato, Augustine, or Luther: the mind is independent and individual, as are body and matter, which, however, are radically negative, the "non-being" subsequently "secularized" by Descartes in his *res cogitans* and *res extensa.*

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I have been asked by the editor to review this book, and thus allow MT to make good its failure to report on this useful work when it first came out. The book is a lucid and riveting semi-popular account of the Chomsky era, and the rather ungentlemanly warfare that began when he tried to suppress the efforts of some of his followers, most notably Postal, Lakoff, McCawley and Ross, "the four horsemen of the apocalypse", to take the theory a great deal farther than Chomsky was willing to see it taken. Chapter 1 tells us that "although the name for their movement, *generative semantics*, has become something of a snide joke in linguistic orthodoxy, one of the aims of this book is to help it regain a bit of its lost virtue–..." The essence of the conflict was Chomsky's insistence that a line "should be drawn between the knowledge of language and the use of language", while his opponents "regard[ed] accounts of linguistic knowledge to be completely artifactual when separated from the application of that knowledge, its use". The many sections begin with colorful quotes from people ranging from Democritus and Augustine to the combatant linguists. Publication dates are always followed by the date of actual writing completion.

After a preface and 6-page introduction which outline the nature of the issues, the 24 pages of Chapter 2 provide an insightful characterization and history of (mostly synchronic) linguistics from the Stoics and the medieval Modistae to Sapir, Bloomfield and Chomsky, contrasting the two earlier Americans with each other, and showing how Bloomfield used the alleged success of behaviorism and logical positivism to justify his exclusion of messy meaning and mentalism from linguistics, providing a concrete, rigorous framework for the investigation of language structure. Chomsky comes along just as the successful structuralists are feeling ready to advance into syntax and semantics, but unsure how to do it. Posing first as a welcome extender of structuralism, he soon shows how his *transformational-generative grammar* completely supplants structuralism in a Kuhnian paradigm shift, to the dismay of his colleagues and the delight of their students. It is rooted in Zellig Harris' (1970 [1952-65]) transformational theory and the spirits of mathematical logic, Carnap's *Logical Syntax* (1937), artificial intelligence, and psychology, which, under Chomsky's influence, developed into modern cognitive science and psycholinguistics.

The next 65 pages, *The Chomskyan Revolution* and *The Beauty of Deep Structure*, outline his substantial contributions. With *Syntactic Structures* (1957), and *The Logical Structure of Linguistic Theory* (1975 [1955]), Chomsky starts by renaming *Immediate Constituent analysis* (the tree-like structure of a sentence, showing how it breaks up, first into clauses, then phrases, then words) as *phrase-structure grammar*, then showing how this system can be made far more parsimonious by viewing all sentences as transformations of certain simple *kernel* sentences (later *deep structures*), e.g., by changing active verbs to passive, or combining several clauses. E.g., from the kernel sentence *Nirm kisses David*, we can get, by alternative transformations,

1 a David is kissed by Nirm.
  b Nirm does not kiss David.
  c Nirm has kissed David.
  d Does Nirm kiss David?

Note that the first transformation does not change the meaning, but the others do. However, if we think of b as being derived from NEG *Nirm kisses David*, then its meaning has not changed either, and c and d can be treated similarly; this preservation of meaning under transformations was termed the *Katz-Postal Principle* (1964). These transformations can also be applied in series to get negative questions, etc. *Generalized transformations* combined simple sentences to make compound/complex ones, so that the latter need not be considered as gratuitous primitive entities, e.g., in 2, a and b combine to c or d.

2 a The cat chased the dog.
  b The cat ate the Kibbles.
  c The cat chased the dog and ate the Kibbles.
  d The cat that chased the dog ate the Kibbles.

All this provides a tool for understanding semantic distinctions like those between the two senses of such ambiguous phrases as "the shooting of the hunters" or "the flying of the planes"; in each case, the two senses have different kernels. The first phrase can come from either 3a or 3b, depending on the sense we choose:

3 a (Someone) shot the hunters.
b The hunters shot (something).

Thus deep structure and transformations give an entrée for representing meaning, an issue which unifies languages, unlike the structuralists' focus on their grammatical idiosyncrasies. In contrast to the latter's emphasis on language diversity, traditional grammar (the Port-Royal *Grammar*, 1975 [1660]), which had tried to uncover the universal features of language, had been on the right track; it merely lacked precision. Chomsky went on to kill the dragon of behaviorism, and revive mentalism and rationalism, showing that the former could never explain the complexities of creativity and language acquisition. His Prague School-influenced ally, Morris Halle (1959b), proved that the theory of the phoneme, "the most beloved of Bloomfieldian results, was made of unfired clay", and replaced it with an extension of Chomsky's theory, *generative phonology.* Bloomfield had ignored important ideas of von Humboldt and others, including the spirit of Jakobson's phonetic universals. (Jakobson, of the Prague School, was the most important theoretical linguist of the mid-twentieth century.)

Chomsky was immediately popular in English studies and psychology, and, with his rationalist publications, in philosophy. However, many were appalled at the viciousness and self-righteousness of the Chomskyans, whose campaign took on the dimensions of "a holy war", with "a gunslinger mentality". His personality was a paradoxical mixture of "graciousness to visitors" and "dismissive hostility" towards anyone he disagreed with.

Aspects of the Theory of Syntax (1965 [1964]), the Chomskyan "New Testament", attempts to deal with issues like the changes of meaning sometimes incurred by transformations, as in

4 a Everyone on Cormorant Island speaks two languages. (not necessarily the same two)
b Two languages are spoken by everyone on Cormorant island. (probably the same two).

Taking refuge in the *Katz-Postal Principle*, that sentences with the same meaning must have the same deep structure and vice-versa, he solves this by denying the difference, on the excuse that a and b only differ in the probabilities they assign to these two interpretations. He also eliminates the need to explain the change in meaning engendered by the interchange of a main clause and a relative clause (generally, it seems, just a change in emphasis or viewpoint *–the reviewer*), as in going from 2d to

5 (2e) The cat that ate the Kibbles chased the dog.

by replacing generalized transformations, which combine two clauses, with recursion within a single clause, i.e., he represents 2d as a clause whose noun phrase contains another clause, 2a.

(It would appear that this reformulation is unnecessary, since the initial order of the clauses being combined by generalized transformation can account for the difference in meaning of the two complex sentences that may be formed from them, and, like ordinary transformations, these generalized transformations do not change the meaning of the original sentences, but merely combine them. In any case, Chomsky's device fails to explain why this particular change in meaning is brought about here, rather than some other, and so is not very profound. Clearly, it is because the main clause is always taken as the departure point. *–the reviewer.*)

(It is interesting that the inability of syntactic deep structure to fully account for the meaning of the sentences in 4 is analogous to Chomsky's criticism of generative semantics' *lexical decomposition* of a word as not yielding the full meaning and connotations of the word, e.g., the meanings of *cowboy* and *snowman* are not fully clear from their morphemes (minimal meaningful components); one must know how the two component concepts are related to each other. Similarly, the components of *crabgrass* do not clearly imply the negative connotation of the latter. It is also analogous to Halle's criticism of phonemic theory[1] as having some exceptions. But these objections are no reason to reject these theories as valueless, for otherwise how can we explain why they *usually* or *approximately* work? This applies as well to the Neogrammarian doctrine of the late 1800's, which insisted that a historical regular sound change rule, such as that deriving Germanic *f* from Indo-European *p* (e.g., *father/pater, five/penta, foot/ pes, pedis*), could have no exceptions; this claim is known to be false, as such a sound change, which spreads over the eligible words during a finite but nonnegligible period of years, need not proceed to completion, but may leave some eligible words unchanged[2] *–the reviewer.*)

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[1] Phonemic theory, often attributed to Jakobson, "crystallized [interactively] in Europe, in Kazan and Prague, about the same time it was crystallizing in America" (the 1930's). It held that the sounds of any language could be grouped into *phonemes*. For the non-linguist, a phoneme can best be described loosely as the set of alternative forms, or *allophones*, that a given sound has when occurring in different positions in a word. In English, for example, *n* has a special throat or root of the tongue (velar) pronunciation when preceding the velar sounds *k* and *g*, as in *ink* and *finger*, while being articulated in all other situations with the tonguetip pressed against the back of the upper teeth, or the nearby *dental ridge* on the upper gum. We say the phoneme *n* has two allophones in English. The possible locations of a phoneme's allophones in a word are always, as with English *n*, mutually exclusive, which is what allows us to interpret them as being the same phoneme whose articulation varies with the phonetic environment.

I will give another example in English, simplified by ignoring the dipthongal nature of the vowel *oo* (as in *moon*) (Gleason, 1955:37), a distinction usually unnoticed by laymen. This example is the phoneme variously spelt as *w* (as in *wall*) or *oo* (as in *moon*). (As always in linguistics, we are concerned with the spoken language only; written representations are derivative and irrelevant, and do not even exist for some exotic languages.) When not occurring near a vowel, as in *moon*, *rude*, *lewd* (American pronunciation), *oops* and *to*, this sound is pronounced with the lips fairly steady (i.e., as a vowel), but when preceding or following a vowel, as in *wall*, *dwell*, or *now*, *loud*, it is pronounced with the lips gliding through a continuum of positions and merging with the vowel (i.e., as a *glide* or *semivowel*). Now, to say, as I have done, that these two (or three, if you prefer) sounds are 'the same sound pronounced differently in different environments' does not strictly make sense, for what do we mean by saying these are the 'same sound'? The concept of the phoneme makes this idea precise by defining a phoneme as a collection of somewhat similar sounds that have mutually exclusive distributions, and can thus be viewed as a single phoneme, adjusted to its various possible environments. What Halle showed is that there is at least one language (Russian) in which there are some words (four, to be exact) which make it impossible to apply the phoneme concept in a consistent way to describe sound distributions in the language, thus vitiating the generality of this concept.

[2] In fact, many such exceptions can be easily explained by phonetic symbolism's conservationist tendency, which requires sounds that iconically represent an aspect of the meaning of the word to remain constant, so as not to lose their iconicity. E.g., although Indo-European *p* becomes Germanic *f*, and the Great Vowel Shift of Middle English changed the high-pitched vowels commonly known as 'short *i*' and 'long *e*' to long *i*, the birdcry *peep* does not change; Latin *pipio* designates a bird that peeps, whence *pigeon* (Jespersen, *Language*:406; Pokorny:830). Both are from PIE (Proto-Indo-European) *pip(p)- 'peep'. Yet English *fife*, a derived but non-onomatopoeic word, shows both changes. (Only words supposedly directly taken from a nature sound are *onomatopoeic*; in contrast, phonetic symbolism, while much more general, is a statistical effect, only working to an incomplete degree. Thus, although a fife is high-pitched, its vowel, taken as a whole, is not as high as that of *peep*, though the second part of this diphthong is still high.)

Such effects are not restricted to onomatopoeic words; e.g., German *weit* 'far' is cognate to English *wide*. Since the lips spread apart laterally in *w*, this phone connotes lateral extent or motion, as in *wafer*, *waffle*, *wall*, *wallet*, *watch*, *wainscot*, *window*, *wheel*, *wake* (of a boat), *wave*, *wag*, *wiggle*, *wobble*, *waddle*, *walk* (an alternation from left foot to right), *wax & wane*; for examples in Chinese, as well as other *w* connotations, see my unpublished paper 'The Wriggly *W*' (1986; available on request). Long/linear items rarely begin in *w* in either language, unless they possess some other *w* connotation as well, such as curvature or oscillation (also due to lip motion or shape), as in *wick*, *weed*, *wheat*, *wand*, *wander*, *walk*.

Since *w* has changed to /v/ in German, the meaning of *weit* 'far' has changed from lateral to longitudinal extent; fricatives (*v, f, z, s, sh*, etc.) and other *continuants* (*m, n, l, r* and short vowels; continuants are the opposite of *stops*, the abrupt sounds *p, b, t, d, k*, and hard *g*.) extend over time in a constant manner, and so connote length, as in *fall, fang, far, fare, fast, fathom, feeler, fiber, file, filament, finger, fiord, firth, fish, flow, flush* (poker), *flute, follow, foot, fringe, furlong, furrow, future.* (Since almost

Aspects also introduced a lexicon in which word meanings are represented as a list (*complex symbol*) of *features*, allowable choices in a given sentence context being governed by *lexical insertion rules*. E.g., in

6 a Avashinee believed her mother.
b *Avashinee believed her canteloupe.

the verb *believe* requires an animate subject and an object that can be "cognized". We say *believe* has the feature + ANIMATE and its object the feature + COGNIZABLE. Thus the empirical distribution of sentences can be described specifying only limited elements of meaning.

All this models speakers' *competence*, as opposed to any specific performance, which may be faulty or influenced by social context. Competence refers to what we know about generating sentences, rather than the process of generating one on a particular occasion. A weakness was that choice of alternative meanings of a sentence, or of two sentences such as 4a,b with the same deep structure, was fobbed off as part of performance, and thus not in need of explanation.

The 34 pages of Chapter 5 describe *Homogeneous I*, the first version of generative semantics, named, only somewhat rationally, in contrast with Chomsky's *interpretive semantics*. In the mid-sixties, a group of Chomsky's graduate students and recent graduates, following cues from Chomsky's writings, realized that there is no real difference between syntax and semantics; syntax is simply that part of semantics which is formalized in grammar. The meanings of morphemes can themselves be represented as *abstract syntax*, a syntax-like structure built up from selected fundamental *sememes*, or maximally simple meaning units. Semantics is *generative*. Lakoff (1976a [1963]) pointed out that, by *Aspects*, the sentence *I like the book* has a different deep structure from *The book pleases me*, despite identical meaning, since *like* and *please* are two unrelated words, thus violating Katz-Postal. But we can derive one of these verbs from the other by a transformation which interchanges subject and object.

Ross (1969b[1967]) considered a similar pair:

7 a Dianna doesn't need to chase the duck.
b Dianna needn't chase the duck.

In *Aspects*, auxiliaries and verbs are different constituents, so a and b have different deep structure, as *need* is a main verb in a, but an auxiliary in b. If we consider auxiliaries as verbs with the added feature + AUX, we are merely switching this feature for *need* from off to on, and a and b have the same deep structure.

Kill, die, dead and *alive* can be united by the *lexical decomposition* of *kill* as *cause to not be alive* (Lakoff, 1970a [1965]:100). Such [*grammatical*] *category-changing* transformations were already necessary, to connect forms like *hard, harden, hardness.* (They're necessary anyways, unless one artificially restricts the realm of linguistics to non-semantic issues.) McCawley (1976b [1967]), generalizing them further, formulated them as *predicate raising*. Many Chomskyans criticized the complexity of the deep structures that arise these ways (one was represented as a mobile in Ross' office), but the Warsaw semanticist Anna Wierzbicka felt they weren't yet complex enough. Harris points out that "This analysis must appear somewhere in the theory, if we are to represent meaning."

Generative semanticists strove to make transformations as general as possible, applying only to broad word classes, and preferably cross-linguistically. In Lakoff's thesis, he generalizes certain highly

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all English *v*- words derive from Latin *w*- words, *v*- words are exceptions to this rule in English; when a sound changes, one may expect words with either the old or new sound's connotations, depending on the particular language and sound change involved. Long vowels and other diphthongs also connote length, but possibly a curved length.) There are also some *f*- words which have the laterality connotation, as *fan, fin, funnel, fen, flat, flap, flag, flake, flannel*, because the phonetic feature dimension *labiality* (lip-made sound), which is orthogonal to the stop–continuant dimension, connotes laterality too, since the lips are part of the face, which is flat (Denofsky, *Studi Italiani di Linguistica* 2003:1, pp. 7-29), but these usually contain *a*, or a reflex (derivative sound, such as long *a*) of it, since this vowel also has the laterality connotation. (The same sound may sometimes have both of two apparently inconsistent connotations, though usually showing just one of them in a given word; ibid.). *As w* is also a labial sound, this provides a second reason why *w* has the laterality connotation.

specific rules of Lees, assigning, for example, the feature [–PASSIVE] ('having no passive') to the verb *resemble* to filter out b in

8 a Jan resembles Mick.
b * Mick is resembled by Jan.

Postal's *Crossover* principle (1971a), far more general, began as an effort to explain

9 a Jeff shaved himself.
b * Jeff was shaved by himself.

One can only derive b from a in two steps, via either the passive transformation to **Himself was shaved by Jeff*, then interchanging the two words that have identical referents, or applying these steps in reverse order, thus passing through **Himself shaved Jeff*. The Crossover principle states that a transformation cannot allow two words with the same referent to cross over each other (thus, in this case, a referent must precede a pronoun referring to it). As b requires two such illegal transformations, it is banned. (Harris does not say whether b is banned in all languages, but even if it is, this may still be an arbitrary choice of little cognitive significance; certainly a perfectly satisfactory language could be designed without this rule.)

(Note that Postal's principle can also handle 4, if we realize that verbs like *resemble* lack passives because their 'object' is actually a subjective completion, i.e., in a sense specified by the word *resemble*, refers to the subject's referent, rather than being acted upon by the subject. The American Heritage Dictionary (1982) illogically calls *resemble* and *equal* transitive, but *be*, and presumably *be the same as*, always intransitive.

However, there also appear to be exceptions to the Crossover principle: e.g.:

10 a Before Andy could get away, he was sprayed with red paint by Mary.
b Before he could get away, Andy was sprayed with red paint by Mary.

In addition, this principle lacks transparency –has no clear rationale. The whole issue illustrates the likely futility of seeking a uniform description, either for individual languages or universally *–the reviewer.*)

Postal's reductionist campaign reanalyzed adjectives and some nouns as deep verbs, as is done by Mohawk and other languages, so that grammar comes to resemble symbolic logic, with sentences modelled as propositions, nouns playing the role of arguments, and verbs predicates, much as, Harris says, Einstein, and now string theorists, have reduced physics to geometry. Thus the supposed Chomskyan goal of fully explaining semantics is achieved, and Chomsky's language-specific syntactic deep structure is replaced by a language-universal semantic deep structure (the McCawley-Ross *Universal Base Hypothesis*), justifying the initial English parochialism of Chomskyans. Forbidden and obligatory transformations differentiating peculiarities of individual languages can be represented as *filters* and *constraints*. Logic thus models the mind, as its inventor, mathematician George Boole, had promised in *The Laws of Thought* [1854].

In Chapter 6, *Generative Semantics II: The Heresy*, Harris notes that Lakoff and Ross developed the first explicit version of the above theory at MIT while Chomsky was away on sabbatical in 1966– "while the cat's away, the mice will play". (Lakoff's earlier 1963 [1976a] proposal had not attracted much interest; attacking Katz and Fodor's (1964b [1963]) and Postal's recent innovations, he had plunged ahead into meaning, "matters that are obscure in the extreme", with a visionary but "arrogant confidence, that Chomskyans were used to seeing in one another's polemics against the Bloomfieldians, but not directed at their own internal proposals".)

Although there was some uncertainty as to how Chomsky would greet the new theory, given his long known ambivalence about shifting linguistics' focus from syntax to semantics, no one was prepared for the shock of his violent rejection in his *Remarks* lectures. To cut the ground from under the feet of generative semantics, Chomsky "repudiated successful early work" that formed the basis for it, including Lees' thesis, which he had directed. Calling his new approach his *revised standard theory*, he advanced the *lexicalist hypothesis*, which, "greatly reduced the heretofore divine right of transformations to change syntactic categories". Semantically related words, even those based on identical morphemes, were no longer related, and a huge, unanalyzable lexicon would be required. "Almost everyone outside MIT, and some inside" (actually Harris repeatedly contradicts himself about the reactions of Chomsky's 'loyal' students), "took the 'Remarks' lectures to be little more than crackpot revisionism."

Chomsky's arguments were "vague, half-baked and ad hoc". In

11 a His criticizing the book before he read it annoyed me.
b *His criticism of the book before he read it was strangely insightful.

he claims that speakers who find b acceptable are reasoning "by analogy", and "are not aware of a property of their own internalized grammar" (Chomsky's words, 1972b). Yet "his dismissive views on the role of analogy were well-known" (1968). His student Jackendoff was left to flesh out his arguments in a series of papers.

One worthwhile contribution of Chomsky at this time was his admission that surface structure can affect meaning, particularly with respect to issues of definiteness (as in Example 4), and quantifiers. (Example 4 recalls Semitic, where, in the default (unmarked) situation, subjects are always definite (taking *the*) and objects indefinite (taking *a*), a not uncommon bias in language, reflecting pragmatic needs *–the reviewer.*) However, his motive here was to retract the Katz-Postal principle, so as to deny it to his opponents in cases like 4. Lakoff, however, countered by pointing out that the *semantic* deep structures of 4a and 4b are different, so that there is no reason why the surface structures should not differ in meaning as well.

While denying that his remarks were anything more than "incidental to generative semantics", he followed, unlike in any earlier period, with "several years of almost exclusively negative rhetoric" (1969:120-202, 1972b:62-119), "whose express aim was to eviscerate generative semantics". He refused to talk with Lakoff and Ross after class, and Lakoff's counterexamples in class led to "[frequent] heated arguments". Lakoff said "Chomsky fights dirty":

> {In one lecture] he took up McCawley's paper on *respectively*, and he put forth the argument that McCawley was arguing against as McCawley's position, and he himself put forth the position that McCawley was arguing for, and he said "See how dumb McCawley is."

Chomsky, however, did not try to interfere with the careers of any of his students at this time. Later, in the early nineties, after long harrassment ("Only [his office-mate] Kenneth Hale was singled out as someone who regularly went to bat for him"), Ross was finally forced to leave MIT in "unpleasant circumstances", and (reviewer's personal knowledge) had difficulty finding employment as a linguist in this country for several years. I have it from Hale (personal communication) that this situation was related to Ross' earning the nickname 'Háj'.

Papers on both sides showed rancor, with titles like "Generative Semantics Methods: A Bloomfieldian Counterrevolution" (Dougherty, 1974) and "Interpretive Semantics Meets Frankenstein" (McCawley, 1976b), but "Most of the explicit enmity… [was] oral", or, often, "mimeographed". At LSA 1969, "for several minutes [Jackendoff and Lakoff] hurled amplified obscenities at each other before 200 embarrassed onlookers". But in an exchange of letters in *The New York Review of Books* as well, occasioned by Searle's 1972 article there on Chomsky's revolution, Chomsky and Lakoff's remarks are filled with "invective", Chomsky's particularly being full of "unsubtle *ad hominems*" like

> Lakoff presents a very confused picture…
> is completely wrong…
> …
> [has] discussed views that do not exist on issues that have not been raised, confused beyond recognition the views that have been raised and severely distorted the contents of virtually every source he cites.

(To be fair to Chomsky, I would reserve the term *ad hominem* to remarks like 'Lakoff is stupid' or 'dishonest', which do not appear here.)

Nevertheless, generative semantics made rapid progress around the country, most notably at the Chicago Linguistic Society, and, in the words of Searle's 1972 review, "Most of the active people in generative grammar regard Chomsky's position as having been rendered obsolete". This attitude was reflected in psychology and literary studies, where Chomsky's theories were either disconfirmed or proved infertile, and, among some "very able critics", the philosopher Dennett.attacked him "with very stinging blows". Although "a tremendously skilled rhetor… His writing can be as dense, gnarled, and forbidding as a blackberry patch, full of fruit you can see but just can't get to, though Chomsky can also reach moments of persuasive lucidity unmatched in linguistics".

Chapter 7, *The Vicissitudes of War*, shows how, over the next 5 to 10 years, the fortunes of the two sides began to reverse. Chomsky's first two attacks– "lexicalism and post-deep structure semantics– failed to resonate with anyone beyond his immediate students." But his third, a reply (1972b) to Postal's 'The Best Theory' (1972a), "hit home". It criticized its "enormous descriptive power", allowing almost unlimited kinds of transformations and *derivational constraints* (*global rules*) (Lakoff, 1970b), With so many parameters that can be adjusted, one can always arrange them to get a good fit, and there is no way to explain why the universal features of language are what they are, rather than something else. Chomsky asked, to explain language, "What kind of rules are needed, if any, beyond those permitted by the standard theory?" Lakoff felt global rules were a simpler concept uniting all the different kinds of mechanisms Chomsky used, but this unity was at the expense of not having a way to specify what rules were actually needed. Almost anything could be proposed as a global rule. It appears Lakoff could have benefitted from some study of Shannon's information theory, well-known in engineering and computer science, and indeed the case against him was confirmed in a series of mathematical papers by Peters and Ritchie. However, Lakoff's advocacy was for global *constraints*, and generative semanticists had actually done more work on this than Chomskyites, until, in the seventies, Chomsky began to accept this and other ideas from his opponents, without ever crediting them (with no one except the generative semanticists appearing to object), and, "curiouser and curiouser– he still maintains [his denunciations]." (Harris does say elsewhere, however, that Chomsky has praised the work of McCawley and Postal.) He made steady progress, and regained the upper hand with *Government and Binding Theory*.

Chapter 8 is a short one that compares the ethos of generative semantics to the concurrent and somewhat extravagant hippie and anti-war movements, in which its adherents seemed to participate in some ways (e.g., consider McCawley's alias 'Quang Phuc Dong'), in contrast to Chomsky, who, though an active anti-war radical, had a style more typical of the restrained, grey-suited establishment. While Chomsky endeavored to insulate syntax from the hairier semantic and pragmatic aspects of language, his more colorful opponents glorified in provocative counter-examples, "data-love", and honest admissions of the limitations of their understanding. They introduced concepts that were difficult to formalize, e.g., Ross' observation (1972c,d) that grammatical categories actually form a near-continuum, or *squish*, a concept which Lakoff nevertheless made some progress with in 1973d [1972] and 1987. The only generative semanticist who ever managed to complete a theory or treatise on it was McCawley's student Judith Levi, who reworked her thesis in 1978, but by this time "nobody was interested". (Levi introduced *Levi extensions*, a semanto-syntactical classification applicable to morphemes and submorphemes, which Rhodes and Lawler (1981) apply to phonetic symbolism *–the reviewer.*) But "It was Chomsky's positive proposals … far more than his negative attacks on generative semantics that pulled his interpretive bacon from the fire, albeit badly singed". It was also found that, in contrast to what generativists had claimed, languages do not share the same deep semantic structure.

Chapter 9 recounts the final "collapse" at this time, when its unfocussed attitude climaxed in a total loss of respect for generative semantics. Its four horsemen, galloping off in different directions, ended up drawing and quartering their theory. Nevertheless, though ignored as a movement in the history books, most of its ideas and results lived on (Chapter 10) as parts of other theories, including pragmatics, or the use of language in actual contexts, ("which dates largely from Ross' early performative work", though originally deriving from Chomsky's introduction of "focus and presupposition"), functionalism (explaining language features in terms of their communicative function), and, above all, *cognitive grammar*, due to Lakoff and Thompson (1975a) and receiving a "huge impetus" from Langacker (1987), who fixed and systematized it in a way Lakoff has been unwilling to do. A more psychologically-based approach, influenced by the connectionism tradition in artificial intelligence (i.e., parallel processing, or neural nets), it became Chomsky's greatest competitor. Generative semanticists had drawn on such broad-ranging sources as Grice's conversational research, Searle's philosophy, the mathematician Zadeh's fuzzy logic, the psychologist Rosch's radial categories, and Goffman's sociology. All went on to do useful and important work that stemmed from their earlier ideas, inspiring a "greening of linguistics", though with little appreciation from syntax-fixated linguists. Ross has progressed logically from pragmatics to poetics (and phonetic symbolism *–the reviewer.*)

Lakoff finally did publish three books, two on metaphor in language and poetry (coauthored; 1980, 1989) and one remarkable one on image-schemas in cognitive grammar, *Women, Fire and Dangerous Things* (1987), which I found highly seminal. Postal and Perlmutter were the driving forces of *relational grammar*. Ross' *island constraints* has received a central place in linguistics, and he was one of those instrumental in relational grammar. Quoting Goldsmith (1989), Harris says Ross' "thesis was the first (and, at the time, mind-blowing) massively cross-linguistic study of an abstract grammatical property, and his conclusions were stated at the level of theory, not that of [a] language-particular property". McCawley, called by Bever "the truest of the true GSers", has created a much-read eclectic and brilliant blend of modern linguistic theories, but "like Sapir, he is not the sort to sponsor a school". And "there are people who would likely prefer another name for [Harris' last] section [Whither Chomsky?], *Wither Chomsky*"! Chomsky's vast influence in linguistics, both in formal modelling and boundary-stretching, cannot, however be denied. In a highly controversial field, I find myself in remarkable agreement with Harris' penetrating evaluations and even-handed judgments. (He is, incidentally, like me, a Canadian.)

One issue that stands out for me from this book is the question of why many linguists assume a final and uniform theory is possible, even of one language. While it is convenient to the user for language to try to base itself on regular structures, the world is supremely irregular, and language in a constant struggle to adapt and change. It would seem unnecessary and very inconvenient for the brain to feel a need to have a perfectly consistent system for it; a capacity for ad hoc adjustments seems more realistic, though some might eventually be absorbed into larger patterns.

At some brain level, though, a more or less uniform, though flexible, implementation should exist, but, like our genes, it need not have the tight efficiency Chomsky demanded of generative semantics. This background unity should also be true at the broader level of the whole brain– compare Langacker (1987:12-13), quoted in Harris:

> ... language has appeared special and unassimilable to broader psychological phenomena mainly because linguists have insisted on analyzing it in an inappropriate and highly unnatural fashion... [We should] integrate the findings of linguistics and cognitive psychology.

Another useful approach would be an indefinitely deep hierarchy of 'exact' theories, ranging from, say, early Chomskyan syntax to a full generative semantics or cognitive grammar with pragmatics, each more comprehensive and detailed than the previous. The prominence or obscurity of a fact or distinction is then measured by how far into this hierarchy one must go till one first encounters it.

While the idea of exceptionless law is supposedly drawn from physics and mathematics, non-mathematicians rarely have a deep enough grasp of these subjects to make such judgments, much less apply them to another field. As a mathematician, it is clear to me that, at a certain level, mathematics is filled with exceptions (such as the inability to divide by 0) and partial plans of organization which cannot be completed without being combined with equally partial complementary approaches. In physics, a prime example of this is the mutually complementary wave and particle theories of matter. Compare Chomsky's concept of the *whole* mind as modular (1978:308), also quoted in Harris (p. 309).

Two better criteria for a language theory might be whether it can help us decipher how the brain works, and guide us in programming a computer to use language. And it's hard to see how either of these tasks could do without a generative semantics; in any system, most concepts must derive from simpler concepts.

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Ever so often, a book appears that breaks new ground, that stands out above all others as a singularly important contribution to the field. Lehmann's *Pre-Indo-European* is such a book. In this book, Lehmann attempts to sketch the fundamental features of an earlier period of the Indo-European parent language. While traditional comparative grammars reconstruct a period just prior to the disintegration of the Indo-European parent language — from around 4500 BCE or so —, Lehmann sets his sights on the period of 8000 to 5000 BCE.

Over the past two decades, there has been a growing recognition that, in its earliest stages of development, the Indo-European parent language was an active language, and this is what Lehmann tries to show as well in this book. Indeed, Lehmann is not the first to make such a claim, nor is the book under review here the first by Lehmann on the subject. The monumental monograph *Indo-European and the Indo-Europeans* (Russian version 1984; English translation 1995) by Thomas V. Gamkrelidze and Vjačeslav V. Ivanov deserves special mention as one of the first to make a convincing case that Proto-Indo-European was an active language. What makes Lehmann's current book special is that every aspect of Proto-Indo-European is carefully examined for residues of earlier periods of development. In his examination, Lehmann brings to bear a knowledge of the relevant literature that is encyclopedic in scope and that clearly reflects a lifetime of learning. His conclusions are compelling.

The book is divided into the following major sections: (1) the bases for reconstructing Pre-Indo-European; (2) from Proto-Indo-European to Pre-Indo-European; (3) residues in Proto-Indo-European that prompt its identification as a reflex of an active language; (4) lexical structure; (5) syntax; (6) derivational morphology; (7) inflectional morphology; (8) phonology; (9) the culture of the Pre-Indo-European speakers; and (10) Pre-Indo-European and possible related languages.

Lehmann begins by discussing the methodologies employed in linguistic reconstruction, noting both the strengths and the weaknesses of these methodologies. In particular, Lehmann stresses the need for a multidisciplinary approach to reconstruction. To set the stage for what follows later in the book, Lehmann (pp. 59—60) describes the salient morphological characteristics of active languages as follows:

> The inflections of active/animate nouns and verbs differ characteristically from those of the stative/animate counterparts in active languages. Active nouns have more inflected forms than do statives. Moreover, there are fewer inflected forms in the plural than in the singular...
>
> Similarly, stative verbs have fewer inflections than do the active...

> As another characteristic verbal inflections express aspect, not tense, in active languages…
>
> Stative verbs are often comparable in meaning to adjectives…
>
> Active languages are also characteristic in distinguishing between inalienable and alienable reference in personal pronouns…
>
> Moreover, possessive and reflexive pronouns are often absent in active languages…

Lehmann then undertakes a rigorous examination and analysis of all aspects of Indo-European in light of these characteristics — this examination and analysis is the core of the book. As a result of this examination and analysis, Lehmann demonstrates that there is strong evidence that there was a distinction between animate vs. inanimate nouns, active vs. stative verbs, involuntary verbs, and particles in an earlier period of development in Indo-European — in particular, Lehmann devotes a great deal of attention to a discussion of particles (pp. 85—99 and 124—130), tracing the development of particles into suffixes, conjunctions, adpositions, and adverbs. In other words, at an earlier period of development, which Lehmann calls "Pre-Indo-European", the Indo-European parent language exhibited characteristics of an active language.

According to Lehmann, Pre-Indo-European distinguished three fundamental stem types: nouns, verbs, and particles. Lehmann discusses in detail the development from this threefold distinction to the more complex system traditionally reconstructed for Proto-Indo-European.

For Pre-Indo-European, Lehmann (pp. 170—171) reconstructs two sets of verb endings that distinguish the active conjugation from the stative conjugation:

	Active	Stative
1st person sg.	*-m	*-χ-e
2nd person sg.	*-s	*-tχe
3rd person sg.	*-t	*-e
3rd person pl.	*-nt	*-r

While I agree fully with Lehmann's reconstruction of an active conjugation distinct from a stative conjugation for this earlier period of development, my views differ somewhat from him on the form of the active endings. First, I believe that *-t was the original form of the 2nd person sg. active ending during the earlier period and that this was later replaced by the ending *-s. The evidence for this interpretation comes mainly from Anatolian and Tocharian: cf. Hittite (2nd sg. pret.) *-ta* in, for example, *e-eš-ta* 'you were'; Tocharian A (2nd sg. athematic) *-(ä)t*, B *-(ä)t(o)*. In the 2nd person pl., however, *-t- is found in all of the older daughter languages: cf. Sanskrit (primary) *-tha*, *-thana*, (secondary) *-ta*, *-tana*; Avestan (primary) *-θa*, (secondary) *-ta*; Hittite (primary) *-teni*, (secondary) *-ten*; Greek (primary/secondary) -τε; Old Latin (primary/secondary) *-tis*; Gothic (primary/secondary) *-þ*; Old Church Slavic (primary/secondary) *-te*; Lithuanian (primary/secondary) *-te*. Next, I view the 3rd person sg. active ending **-t* as a later replacement for original **-s*. That this replacement occurred fairly early is shown by the fact that the 3rd person sg. ending **-t-* is found in all of the older daughter languages, including the Anatolian languages. However, there are important indications that **-s* was the original 3rd person sg. active ending. Residues are found not only in Hittite and Tocharian but in the other daughter languages as well, especially in the sigmatic aorist. The evidence is discussed at length by Calvert Watkins in his books: *Indo-European Origins of the Celtic Verb: I. The Sigmatic Aorist* (1969; Dublin: The Dublin Institute for Advanced Studies) and *Indogermanische Grammatik. Band III: Formenlehre, Erster Teil: Geschichte der Indogermanischen Verbalflexion* (1969; Heidelberg: Carl Winter, Universitätsverlag). Finally, I consider the 3rd person pl. active ending **-nt* to be a compound in which **-t* has been added to original **-n*. The **-t* was added to the 3rd pl. ending **-n-* at the same time that **-t* started to be used in the 3rd person sg. Though I use different symbols for the stative endings, my views are identical to those of Lehmann. Thus, I would reconstruct the Pre-Indo-European verb endings as follows:

	Active	Stative
1st person sg.	**-m*	**-A-e*
2nd person sg.	**-t* (later **-s*)	**-tAe*
3rd person sg.	**-s* (later **-t*)	**-e*
3rd person pl.	**-n* (later **-nt*)	**-r*

Though Lehmann attempts to reconstruct Pre-Indo-European verb morphology in some detail, far less attention is paid to the reconstruction of Pre-Indo-European noun morphology. He does note, however, that considerably fewer case endings existed in Pre-Indo-European than are traditionally reconstructed for Proto-Indo-European and that few, if any, case endings existed in the plural (p. 184), and he does outline the development of noun inflections (pp. 183—186) from a simple to the more complex system. Lehmann also notes (pp. 187—188) that adjectives originally did not exist as a separate grammatical class in Pre-Indo-European, and, inasmuch as this agrees with the patterning found in active languages, it provides another piece of evidence that Pre-Indo-European is to be reconstructed as an active language. Finally, Lehmann devotes whole chapters to an examination of lexical structure (pp. 64—99), to syntax (pp. 100—133), and to derivational morphology (pp. 134—166).

It is gratifying to see that Lehmann (pp. 198—202 and 211—214) now accepts a form of the Glottalic Theory. Lehmann reinterprets **b*, **d*, **g*, *g^w of traditional Indo-European as *'*p*, *'*t*, *'*k*, *'k^w respectively, with preglottalization. Furthermore, he reinterprets the traditional plain voiceless stops and voiced aspirates as voiceless and voiced respectively with aspirated and unaspirated allophones. As in his earlier work (*Proto-Indo-European Phonology* [1952; Austin, TX: The University of Texas Press], pp. 100—102, §13.3), Lehmann (pp. 214—216) posits only palatovelars and labiovelars for Proto-Indo-European, assuming a secondary status for the plain velars reconstructed by the Neogrammarians. Lehmann reconstructs the following four laryngeals: *?, *h, *χ, *γ. Lehmann assumes that *χ and *γ were voiceless and voiced velar fricatives respectively and that *γ may have had a *w*-offglide. Lehmann's revised reconstruction of the Proto-Indo-European phonemic system is as follows (p. 201):

Vowels

ī								ū
	e	ē		ə		o	ō	
			a		ā			

Consonants

	Obstruents			Resonants				Fricatives		
Labials	p	p'	b^h	m			w			
Dentals	t	t'	d^h	n	r	l	y		s	
Palatovelars	k	k'	g^h							
Labiovelars	k^w	k'^w	g^{wh}							
Laryngeals	?							χ	γ	h

For Pre-Indo-European, Lehmann reconstructs the following phonemic system (p. 218):

	Stops			Resonants				Fricatives		
Labials	p	'p	b^h	m			w			
Dentals	t	't	d^h	n	r	l	y		s	
Palatovelars	k	'k	g^h							
Labiovelars	k^w	$'k^w$	g^{wh}							
Laryngeals	?							χ	γ	h
Syllabicity										

My own views are close to those of Lehmann in that I would only reconstruct three series of obstruents for Proto-Indo-European: (1) voiceless (aspirated), (2) voiced (aspirated), and (3) glottalized. I agree with him that only (palato)velars and labiovelars are to be reconstructed. I would also posit four laryngeals, though I differ slightly in the phonetic values I would assign to two of the laryngeals — I interpret the laryngeals (using Kuryłowicz's symbols) *ə̭$_2$ and *ə̭$_3$ as multiply-articulated voiceless and voiced laryngeal-pharyngeal fricatives respectively: *ə̭$_2$ = *ħ͜h (Lehmann's *χ) and *ə̭$_3$ = *ʕ͜ɦ (Lehmann's *γ). In my opinion, positing laryngeal-pharyngeal fricatives here makes it easier to account for the vowel coloring effects usually attributed to these laryngeals. Finally, I would reconstruct a full set of vowels for the latest period of development as well. Thus, I would reconstruct the Proto-Indo-European phonological system as follows:

Obstruents:						
Voiceless (aspirated):			p^h	t^h	k^h	k^{wh}
Voiced (aspirated):			b^h	d^h	g^h	g^{wh}
Glottalized:			(p’)	t’	k’	$k’^w$
				s		
Laryngeals:	ʔ	h	ħ͜h̲ ʕ͜ɦ			
Resonants:	m/m̥	n/n̥	l/l̥	r/r̥	w/u	y/i
Vowels:	e ē	o ō	a ā	(i) ī	(u) ū	ə

I concur with Lehmann (pp. 209—211) that Proto-Indo-European passed through several successive periods in the development of ablaut and accent. Indeed, my views are, to a large extent, derived from his views. Where I disagree with him is in the reconstruction of the feature of syllabicity, without further differentiation, as the nucleus of a syllable in Pre-Indo-European. My own research indicates that the Proto-Indo-European vocalic system underwent a complicated series of changes in the course of its development. These changes can be traced fairly accurately, even if all of the details are not yet completely clear. It can be shown that there was never a point in its prehistory that Indo-European did not have a full complement of phonemic vowels, though, it goes without saying that the sets of vowel phonemes to be reconstructed for the earlier periods were not identical with the sets of the later periods. For details on my views, cf. Allan R. Bomhard and John C. Kerns, *The Nostratic Macrofamily* (1994; Berlin and New York, NY: Mouton de Gruyter), pp. 73—85.

Lehmann rounds out the book with a description of the cultural setting of Pre-Indo-European (pp. 219—245). Lehmann examines both linguistic and archeological evidence (pp. 221—223). He identifies the terms for common household animals (pp. 228—232), the social and economic conditions (pp. 223—226), and the terminology indicating gradual development from a hunter-gatherer society to a settled society (pp. 232—236). He places special importance on the role of tokens in non-Indo-European societies of the ancient Near East and their lack of use among the Pre-Indo-Europeans (pp. 236—239). On this basis, Lehmann rejects the idea of a Pre-Indo-European homeland in Asia Minor (pp. 238—239). He favors a homeland in the steppe area to the north of the Black and Caspian Seas — a view I wholeheartedly endorse. Next, Lehmann discusses art, literature, and religion in Pre-Indo-European (pp. 239—241) and life in the Pre-Indo-European period (p. 241). He ends with an account of how and why Indo-Europeans expanded outward from their original homeland and gained dominance over a vast region stretching from Europe in the west to Iran, India, and Central Asia in the east (pp. 242—245).

The final chapter in the book focuses on Pre-Indo-European and possible related languages. Lehmann mentions specifically the Nostratic Hypothesis and the proposals of Joseph H. Greenberg, according to which Indo-European is assumed to be a member of the putative Eurasiatic language family. Lehmann's work is especially valuable here, since it lays a better foundation for comparison with possible related languages than what is found in traditional comparative grammars.

Lehmann is noted for his ability to present complicated ideas in a clear, easily accessible manner, and this book is no exception — this is a well-written and carefully edited book. I found few typos, though it should be noted that there are a number of works cited in the body of the text that are missing from the list of references. ■

Are Linguists Inherently Conservative?

I am not using the C word in its political sense but in its ordinary and original sense of one who wants to conserve things as they are, whatever the realm of activity she may be in, rather than wanting to change things or to accept changes that have been made. It could be argued that conservatism is an attitude of contentment; 'I am happy with things as they are.' I had a surprise recently to hear a Supreme Court Justice, one Stephen Bryer, and one normally classified as one of the 'liberals' on America's highest court, say the following: "We are all conservative, each of us is conservative with respect to some points of the law or the value of the laws which hold our society together."

There are some linguists who are politically liberal and some who are politically radical, i.e., wanting to make changes and advocating change. Noam Chomsky comes to mind. It is fair to say that most anthropologists are either liberal or radical politically, yet we ethnologists are famous for being very conservative with respect to 'my people' whose culture the outside world is trying to change. So we may say that political attitudes do not necessarily correlate well with other attitudes.

In some ways the finest part of linguistics is the Indo-European community, those who study the 144+ languages and dialects of Indo-Hittite and communicate their results to each other. Many fine colleagues, many bright colleagues, enrich ASLIP with their presence. Their particular (peculiar?) methods are usually accepted as the norm for Historical Linguistics, all summed up in the semi-sacred 'Comparative Method' Or to put the matter in a somewhat different way it is the Indo-European community which constitutes the jury and the judges in deciding the fate of any long range hypothesis involving language. Let the Greenbergians applaud it, let the Russian Nostraticists cheer for it, it makes no difference, unless the Indo-European community accepts it. Does anyone dispute the truth of the last sentence? Please let them tell me about it!

Why mention this in the middle of a fine review of a very good book? The question comes up because Lehmann's book aims at the prehistory of Indo-European before it became proto-Indo-European (PIE). You might say he is trying to maximize the extent to which 'internal reconstruction' of PIE can reveal some earlier state of being. Well and good; everyone would approve of that. But why not look a little deeper? And since Greenberg's Eurasiatic or Bomhard's Nostratic or the Moscow version are surely well known to him, why not examine PIE's relationship to them? In a book of nearly 300 pages there was no room for Eurasiatic or "Indo-European's closest relatives"? Evidently Lehmann chose to ignore those hypotheses because they were ....what? Too fragile? Too bold? Unthinkable? Not yet accepted by his colleagues? Or perhaps it would require too much commitment of time and energy to bring them up for consideration?

My considered opinion is that Lehmann and the Indo-European community are contented with things the way they are. They exude no hostility towards long rangers, unlike the Americanists, but they just don't want to change. They got a good thing going so why rock the boat? Bomhard as a Nostraticist seems nevertheless to understand this because he never brings up the subject in his review. Overleaf he answers my questions about the content and closer definition of some terms but he respects, indeed honors, the conservatism of the book. So be it.

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Sunday, March 06, 2005

Dear Hal:

I was surprised to find that I still had the old envelope that I created for ASLIP many, many years ago, so I used it. On another note: I am glad that I got to speak with you this week. I had forgotten how much I enjoyed our conversations!

Now let me turn to the questions you asked. I cannot speak for Lehmann -- it is he who chose the terminology "Pre-Indo-European". A better choice, perhaps, would have been "Pre-Proto-Indo-European", because that is what he is discussing.

The traditional comparative grammars attempt to reconstruct the parent language ("Proto-Indo-European") on the basis of a direct comparison of the attested daughter languages. That parent language is usually dated to ca. 4500 B.C.E. For the most part, that endeavor has produced widely-accepted results, even though there are still some areas that are contested. The question arises then "what came before that?" Many (Ken Shields, for one) have thought in terms of ergativity as a precursor to the nomative-accusative structure of traditional Proto-Indo-European.

Recently, a growing number of scholars have put forth compelling evidence that the ancestor of Proto-Indo-European (Lehmann's "Pre-Indo-European") was a language with an active-type structure (such as Thomas Gamkrelidze, Vjačeslave V. Ivanov, Andrew Sihler, and Winfred P. Lehmann, among others). Elamite comes to mind as an active language (at least according to Margaret Khačikjan, *The Elamite Language*, Rome: 1998). Proto-Afrasian is also assumed to have been an active language (cf. Igor M. Diakonoff, *Afrasian Languages*, Moscow: 1988, p. 85). In active languages, the subjects of both transitive and intransitive verbs are semantically agents and are treated identically for grammatical purposes, while non-agent subjects and direct objects are treated differently (cf. Larry Trask, *A Dictionary of Grammatical Terms in Linguistics*, London and New York, NY: 1993, pp. 5—6). Trask also notes that:

> The correlation is rarely perfect; usually there are a few verbs or predicates which appear to be exceptional. In some active languages lexical verbs are rigidly divided into those taking agent subjects and those taking non-agent subjects; in others some lexical verbs can take either to denote, for example, differing degrees of control over the action.

In his book, Lehmann (pp. 59—60) provides a particularly clear description of the salient morphological characteristics of active languages:

> The inflections of active/animate nouns and verbs differ characteristically from those of the stative/animate counterparts in active languages. Active nouns have more inflected forms than do statives. Moreover, there are fewer inflected forms in the plural than in the singular...
>
> Similarly, stative verbs have fewer inflections than do the active...
>
> As another characteristic verbal inflections express aspect, not tense, in active languages...
>
> Stative verbs are often comparable in meaning to adjectives...
>
> Active languages are also characteristic in distinguishing between inalienable and alienable reference in personal pronouns...
>
> Moreover, possessive and reflexive pronouns are often absent in active languages...

Even within Proto-Indo-European itself, we are now getting a glimpse of a morphological structure quite different from what was reconstructed for the parent language by the Neogrammarians (Karl Brugmann and others). Just to look at verbal morphology, for example, we can say with complete confidence that the dual number did not exist in the Early Proto-Indo-European verb — it was a later formation. Simple thematic verbal stems may also be tentatively regarded as later formations. It appears that they were just beginning to develop at the time when the Anatolian languages separated from the main speech community. We should note, however, that, except for the 1st person singular, the personal endings of the thematic stems were identical to those of the athematic stems. As in Hittite, there were at least two tenses (present/future and preterite [= non-present]), two moods (indicative and imperative), and two voices (active and middle). The preterite was originally neutral as to tense. There were two contrasting superordinate aspectual categories (dynamic and stative). The dynamic aspect referred to actions and processes, while the stative aspect referred to states. There was also an iterative aspect.

Thus, the so-called "perfect" of traditional Indo-European comparative grammar is now to be re-interpreted as stative. It referred to a state in present time and was restricted to verbs that were semantically appropriate. Later, it developed into a resultative and, from that, into a preterite in the individual non-Anatolian Indo-European daughter languages. The perfect was characterized by reduplication, by a special set of personal endings (listed by Lehmann on p. 171 of his book), and by a change of accent and ablaut between the singular and plural. There was no distinction between "primary" and "secondary" personal endings in the perfect.

I could go on and on, but I think you get an idea of how things have changed in Indo-European studies.

As for the homeland question, Lehmann supports the view that the Indo-European homeland is to be located in the area to the north of and between the Black and Caspian Seas. He rejects the views of Renfrew and his followers.

Please add this as part of your Editorial Comments. Thanks.

With best wishes,

Allan

***Languages and their Speakers in Ancient Eurasia: Dedicated to Professor Aharon Dolgopolsky on his 70th Birthday*.** **Edited by Vitaly Shevoroshkin and Paul Sidwell. AHL Studies in the Science and History of Language 5. Canberra: Association for the History of Language, 2002. 269 pp.**

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This book is a very informative and useful collection of twelve articles that offer significant updates on the status of comparative linguistics in Eurasia. It includes prefaces by both of the editors, a collection of pictures of several of the individual authors, and several indices (author, citation, subject, language, and word). It is divided into three subsections, which will be discussed in order below.

Part I: Phonology and Grammar is 41 pages long. Its first article, Sergei Starostin's "Nostratic Stops Revisited," examines a new fourth row of Nostratic initial stop correspondences based on evidence from Indo-European, Kartvelian, and Altaic:

	Indo-European		**Kartvelian**		**Altaic**		**Nostratic**	
(a)	*th	*kh	*t?,	*k? (q?)	*th	*kh	*th	*kh
(b)	*t	*k	*t,	*k	*t	*k	*t	*k
(c)	*d	*g	*d,	*g	*d	*g	*d	*g
(d)	*t	*k	*d,	*g	*th	*kh	*dw	*gw

The new series is shown in (d), which Starostin interprets as voiced labialized stops, arguing that devoicing of this series is typologically usual. He gives a total of ten etymologies that show this correspondence. The paper is brief, but foreshadows further progress in Nostratic phonological studies.

John Orr's "Does the Indo-European Suffix -TER Come From Uralic?" sets out the hypothesis that the Proto-Indo-European relationship suffix -TER may be decomposed into -TE + R (ultimately from ÄTJÄ 'father' +R), and that the shape of the PIE kinship terms can be explained through the suffixation of -TER to more basic terms which have cognates in Uralic. For example:

Uralic				**PIE**			
PÄÄ	'head'			PA	'father, head'	>	PA+TER
EMÄ	'mother',	IMÄ	'breast'	MA	'mother'	>	MA+TER

Orr concludes with a brief discussion of the extension of the suffix -TER to other nouns such as *aviator* and *refrigerator*.

In "The Role of Morphology in Nostratic Studies" Peter Michalove presents a brief but poignant discussion about the importance which morphological evidence must play in testing the Nostratic hypothesis, concluding that the best case scenario will involve the reconstruction of a total morphological system, not just individual grammatical morphemes.

Claude Boisson compares eleven grammatical parallelisms between Sumerian, Nostratic and Sino-Caucasian in "Some Sumerian Grammatical Elements in a Nostratic Perspective." He considers it possible that Sumerian might have been related to both Nostratic and Sino-Caucasian, but based on his work he considers the relationship with Nostratic to be closer. Of the eleven grammatical parallels that involve Sumerian, all of them include Nostratic counterparts to Sumerian morphemes, but only six include Sino-Caucasian counterparts. Each entry discusses the Nostratic and Sino-Caucasian subgroups, giving examples from the daughter families of each where possible. The eleven grammatical categories treated are negation, interrogative, reflexive, cohortative, demonstrative (3), animate noun plural, coordinative / comitative, coordinative / emphasizer, and ablative-locative.

Part II: Etymology is twelve pages long, and the shortest section of the book. It begins with Aharon Dolgopolsky's "Three Entries from the 'Nostratic Dictionary'." The three lexical items which are covered are 'forehead, front', 'to spread like a veil/net, cover with a veil/net, catch (fish, etc.) with a net', and 'place'. Dolgopolsky's entries are both thorough and necessarily very dense, and I was thankful that he provided a symbol key at the end of the article, as symbols abound throughout the entries. This article is not sufficient, nor intended, to give any concrete proof for Nostratic based on sound correspondences or the like, but it is an excellent example of the kind and quality of work which we can expect from the Nostratic dictionary *en totale* once it is finally completed. Both supporters as well as skeptics should find this article useful in judging Dolgopolsky's handling of the Nostratic data.

John Bengtson's "Dene-Caucasian *X$^{(w)}$owHV 'Mouth ~ Tooth'" offers a case study of one Dene-Caucasian (DC) lexical item, which he provides cognates for in the following four branches of DC:

Proto-Basque	**Proto-Sino-Tibetan**	**Proto-Yeniseian**	**Proto-Na-Dene**
*-h^{w}o/-x^{w}o 'mouth'	*Kho(w)H 'mouth'	*Xowe 'mouth'	*Xu:? 'tooth'

Bengtson notes that reliable cognates have not been found in either North Caucasian or Burushaski. He provides a discussion of how he reconstructs the Basque Proto-Form, the forms for the other three branches being reconstructed by Starostin (Sino-Tibetan and Yeniseian) and Pinnow (Na-Dene). At the end of his article, he offers a very reasonable explanation for the divergence in meaning between 'mouth' and Na-Dene 'tooth' as resulting from synecdoche.

Finally, in his one-page "Nostratic Naming of the Index Finger," Vladimir Terent'ev suggests that the Indo-European word for 'four' **kuetuer* and the Finno-Ugric word for 'six' **kutte* might be reconciled if it is assumed that different styles of counting on the hand had been used in the two communities. He suggests the possible proto-form **k'utV/q'utV*.

Part III: Lexicon comprises the bulk of the volume, and spans two hundred pages. The first three articles of this section are authored or co-authored by Harald Sverdrup. In his first article, "The Pictish Language," Sverdrup provides a comprehensive discussion of the textual materials available to him on Pictish, his translation methodology and philosophy, and the general state of Pictish studies. His structural analyses include noun inflection, the verb, pronouns, and ethnonyms/toponyms. Sverdrup argues that structural properties such as agglutination, ergativity, and a noun class system, as well as lexical items such as numerals and parts of the core vocabulary and a tradition of matrilineal succession, show that Pictish is decidedly non-Indo-European, but shows significant similarities with Basque and Iberian, as well as a certain similarity with Etruscan and North Caucasian.

Sverdrup's second article, "Exploring Properties of the Rätian (Rhaetic) Language," provides a similar treatment of the prehistoric Rätian language which was spoken in Eastern

Switzerland and northeastern Italy. Although data on Rätian has been rather sparse, Sverdrup is optimistic about new developments within the field. Sverdrup compares Rätian with Etruscan and Lemnian in particular, comparing the structures of the noun, verb, and adjective, and argues that the translation of several Rätian texts becomes possible if the *a priori* assumption is made that it is related to Etruscan. The possibility of a relationship with Basque and North Caucasian is also discussed.

Sverdrup's third article, "A Study of the Tartessian Script and Language," is co-authored with Ramon Guardans, and is an analysis of the Tartessian language spoken formerly in the Iberian peninsula. There is once again a good discussion of methodology and of the existing transcriptions, and the authors provide a significant reanalysis of the Tartessian script. Among the conclusions at the end of the article are that Tartessian is similar to the Iberian language in its suffixes, but different in its glossary, and that while it seems reasonable to conclude on the basis of the current evidence that Tartessian was non-Indo-European, there is still insufficient evidence to make a strong case for its relationship to languages such as Basque, Etruscan, Iberian, or what is termed Paleoeuropean.

Vyačeslav Ivanov's "Comparative Notes on Hurro-Urartian, Northern Caucasian and Indo-European" is the lengthiest contribution to this volume. The main purpose of this article is to "enrich Hurro-Northern Caucasian comparison" based on the discovery of a large Hurrian-Hittite bilingual text and other results of recent excavations. Ivanov's discussion is definitely not for the layperson, and involves a number of highly technical arguments and a very involved presentation on five different lexical areas and one morpheme: *king*, *fire*, *slave/boy*, *horse* (the most extensive discussion by far), *wheeled chariots and related objects*, and the *voluntative in –l-* as a possible areal feature. Ivanov's scholarship and attention to detail are to be commended, although his presentation is quite dense; on balance, it makes a valuable contribution to the field of Hurro-Urartian philology and comparative linguistics.

Finally, Claude Boisson's second contribution, "Sumerian Terms for Caprines and Antelopes" delivers an exposition of four Sumerian words for ovines, caprids, and gazelles. He examines potentially related lexical items in several language families including Afroasiatic, Dravidian, Finno-Ugric, Bantu, and North Caucasian, as well as Urartian and Elamite. The second half of his paper includes a discussion of the archaeological record, and his main inference is that the words for these animals were probably diffused widely with neolithization from some part of the Near East, cautiously suggesting an Afroasiatic source.

It was a pleasure to read this volume, which contains a large amount of exciting data and discussion. Although the thematic breadth of the articles is rather wide in some ways, this does not detract from its overall utility; it does imply, however, that it will appeal to a somewhat varies audience, and any one reader will probably find some sections and articles more useful than others depending on their personal inclinations. The reader who expects fully developed discussions of all of the represented topics may be disappointed, as several of the articles are rather brief and intended more to give a sample of the research they report on instead of a thorough report and/or analysis. Having said this, the reader's imagination and interest should be readily stimulated by the avenues for further exploration that are opened by the articles in this book.

ANNOUNCEMENTS

ADVERTISEMENTS

NOTES

Basquology

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In an optimistic way, one might even call it exaggerating (Klemperer 1969: 218 would have termed it "Americanism" – though not restricted to U.S. citizens/institutions), which naturally is a tourist brochure's business, the benevolent visitor to Juneau is informed that "Southeast Alaska has been home to the Tlingit... for tens of thousands of years".

Looking for some scientific "truth," I came upon Starostin (1989: 43) telling us that Dene-Caucasian has been dated "approximately to the 9th and 8th millennia B.C.," with a subsequent division into an eastern (Mongoloid) and western (Caucasoid) part, the latter linguistic group comprising Basque, Caucasian, and Burushaski being named "Macro-Caucasian" by John Bengtson (to which "Paleo-European" languages, all extinct, even including Etruscan, may have belonged).

"Paleo-European" here therefore does not correspond to Theo Vennemann's "Old European" (i.e., "Vasconic" languages related to modern Basque). Even his time-table does not agree with Starostin's as the Old Europeans already after the last Ice Age moved from southern France into the rest of the continent (see Vennemann *et al.* 2002 for the latest state of the art): By 16,000 B.C., Vasconic people – having originally come from the Near East *before* the Ice Age had reached its maximum and having survived it in southwestern Europe – invaded central Europe, which was unpopulated. The West and North of the continent were settled by 15,000 - 10,000 years ago (cf. the Vasconic substratum in place-names all over Europe; e.g., *aran* 'valley', **is-* 'water, body of water').

Thus, in order to satisfy every aspect of Dene-Caucasian that has been put forward so far it seems necessary either to change the timing or the homeland theory (for both Dene-Caucasian and Macro-Caucasian). As for time then, maybe even the Juneau tourist brochure, if it would alter the text to "ten thousand years," might not be that far wrong . . .

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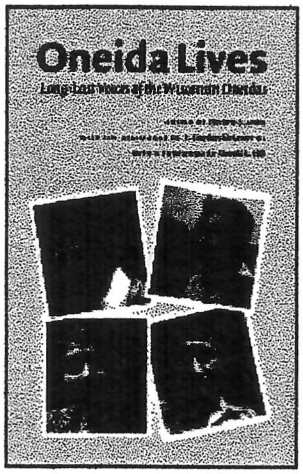
BOOKS



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Edited by Herbert S. Lewis with L. Gordon McLeste
by Gerald L. Hill

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Herbert S. Lewis is a professor emeritus of anthrop
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the Oneida Nation of Indians of Wisconsin and the
Oneida Indian Journey: From New York to Wiscons
Gerald L. Hill is a member of the Oneida Nation of
former chief counsel and special counsel for the On

A BOOK PRESS FOR ASLIP

Murray E. Denofsky

Hal Fleming has encouraged me to present a proposal of his for solving a major publishing dilemma facing members trying to publish books giving a full description of a language. Since Chomsky's issue-oriented approach to linguistics became dominent, only reports on restricted aspects of a language, such as its phonetics, have been published in the US, and university presses no longer accept language descriptions. Even cooperative presses founded by groups of linguists to handle this problem do not make themselves generally available, and ASLIP members have to go to Europe, principally Germany, to secure publication. While the German presses, such as Otto Harrassowitz, provide good publicity, allowing wider sales than a linguist could arrange for on his own, they do not give royalties. Sales without adequate marketing, like those of Mother Tongue, which goes only to ASLIP members, are insufficient to do more than barely cover expenses.

If members are to achieve broad readership and make some profit to reward their diligent research, it will be necessary for ASLIP to make an organized effort to pool the marketing savvy of all its members, so that a book press we found would be able to reach a broader audience than members alone. We hereby call for all members with any such useful knowledge to contact MT and volunteer such information, expertise, and relevant proposals, which I will collect and jointly evaluate with the Board. Hopefully, this will make a profitable book press at ASLIP feasible.

Any useful ideas should be either e-mailed to mur251@yahoo.com, or mailed to

Murray E. Denofsky
252 Medford St. #809
Somerville, MA 02143.

SOME CRITIQUE OF LONG-RANGE COMPARISON BY KEVIN TUITE

By Murray E. Denofsky (mur251@yahoo.com)

In a 40-minute conversation with Tuite in his office at the Université de Montréal on 8/12, he maintained a number of criticisms of, and advice for, ASLIPers, in the face of my recounting of common pro-ASLIP policy rationales. He feels that it is no virtue that ASLIP claims to use traditional sound change analysis, as our more distant comparisons require more than these IE studies methods. He feels typological differences, such as those studied by Johanna Nicholls, have a greater time-depth stability and are more convincing. (I know Greenberg did typology studies.) These and grammar comparisons should be done to determine relatedness of languages before vocabulary is compared. He feels that in Greenberg's Altaic work, two-thirds of the data is faulty. He feels Witzel's knowledge of Sanskrit is exemplary, but would like to see him more critical with other language data. ASLIP people are often not familiar in detail with the languages they are comparing. Quality controls are needed, some way of estimating the credibility of the results, but glottochronology is unreliable.

In the New World, while the Eskimo and Na-Dene phyla hold water, similarities among Amerind families may be due to 'borrowed phonetic symbolism' (See Denofsky in *Studi Italiani di Linguistica* 2003:1 for a discussion of evidence of borrowed symbolism among English, French and German, in contrast with Greek and Russian, although this is admittedly borrowing among closely related and nearby languages, and its lack of occurrence in Greek and Russian lends no support to Tuite's more far-reaching suggestion.) Tuite also feels that, as the Bering land bridge was open for a considerable time, it is likely that many different language phyla crossed, considering that even today there are 4 phyla in Siberia, and others have probably died out. News of new big game hunting opportunities 'spreads rapidly'. He feels that these are the reasons that most linguists are not impressed by most long-range work. The editor of *Mother Tongue* has informed me that he does not credit these objections.

I was impressed by his familiarity with the phonetic symbolism literature. He gave me copies of two current papers of his, *Of Phonemes, Fossils and Webs of Meaning: The Interpretation of LanguageVariation and Change*, 38 pp. + 17 pp. bib. (to appear in an as yet unnamed book ed. by Christine Jourdain & K. Tuite), and a review article in *Historiographia Linguistica* XXX:1/2 205-217 (2003), *Explorations in the Ideological Infrastructure of Indo-European Studies*. He would be pleased to receive our comments on these papers at tuitekj@anthro.umontreal.ca. I would be happy to xerox my copies for you, or, I imagine, you could ask him to email them to you.

SPECIAL VENTURES: INNOVATIVE MAPPING

By William G. Davey, PhD
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Belen, New Mexico 87002
505-864-4499

South American (Linguistic) Migrations
(including Special Maps in Color, As Detachables)

William Davey is a **physicist,** trained in the UK and long associated with the laboratories at **Los Alamos**, New Mexico. He is the third physicist who I know to be associated with our work – long ranging. The first, and a founding member of ASLIP, was **Ron Christensen** of Lincoln, Massachusetts. He was followed by **Murray Gell-Mann** of Santa Fé, New Mexico. There are several others who are either engineers or physicists – they have not yet said which they are – living in the Netherlands and Italy. My apologies to any who I have mis-remembered.

Physicists bring a lot to our table, to our endeavor. Most of all it is **confidence**, mother of boldness, which they contribute. Secure as scientists and already successful scholars in a highly technical discipline, they feel free to explore new alleys, to go out on limbs, to see where the wind will take them. I once felt that way-- after I had mastered the Multiplication Tables! But there is more to it than that because physicists have (we hope) mastered the most **prestigious** and oldest of all the sciences. (Perhaps geography could debate the 'oldest' bit). No social science, except economics, is awarded a Nobel Prize. So why would a physicist feel a need to take any baloney from second rate scientists? Why follow their alien rules which don't seem to be doing much good anyway? Why not look at their problems and find better solutions for them? Why not help them? Remarkably enough, despite their potential, physicists have nonetheless screwed things up sometimes; archeology has been the scene of some egregious cases. (There will be no specifics.) Yet archeology has seen the greatest benefits from contributions of physicists. Chronology is nowadays beholden to physics for precision and great reach in years. Almost but not quite dependent on it.

What William Davey is doing for us is to look at that **bewildering mass** of languages covering a whole continent and make prehistoric sense of it. South America should be called South Chaos for the complexity apparent when the languages are all put together on the same map. Mind you, he is **not** making **genetic** sense out of the languages; Greenberg **already** did that. Bill is liberating **the prehistory** contained in the languages and their distribution by inferring migrations from the present confusion. This is not Dispersal Theory, not simplistic 'find the center' theory, not really following any of our linguistic rules. I think it is brilliant, certainly creative, surely useful, and for all those reasons we have gone to the trouble of offering it to our members in the only form which will really communicate the conclusions to them – IN COLOR. Enjoy the maps!. The individual pages are detachable from the book so that they can be easier to use.

South American Migrations

William G. Davey

Abstract

The present location of languages in South America shows a very complex pattern where the eight different groups are scattered in a fragmented manner over most of the continent. But the separated regions in which each group are now located must have had some previous connection and we assume that an envelope drawn around the present locations of all areas of a given group gives the possible minimum previous extent of that group. That is, we assume that the remnants of an earlier distribution are essentially "fossilized in place". We have successively "unfolded" the present pattern of languages as given by Greenberg with a series of maps to the point where the remaining groups do not overlap or intrude upon each other. This procedure results in two such "earliest" groups – Andean and Macro-Panoan which extend from the northwest toward the south. Following this the Macro-Tucanoan expanded from the upper Amazon Basin to the Atlantic with some small extensions into the Andean and Macro-Panoan areas. These three groups appear to constitute the earliest occupation of the majority of the country in three almost independent movements. This pattern was then significantly disrupted by the southward expansion of a combined Chibchan-Paezan group which probably moved south from the region of the isthmus of Panama in two narrow migrations – down the west coast and also very deeply south through the center of the country. This was then followed by a broad expansion of Macro-Carib, probably from the central part of the northern coast, to the west and south. This disrupted the Chibchan-Paezan and further disrupted the already fragmented Macro-Tucanoan peoples. A subsequent expansion of the Equatorial peoples – probably from the general coastal region of southern Brazil – overwhelmed the southern Macro-Carib peoples and further fragmented each of the other four groups to varying degrees. The final expansion was of the Macro-Ge speaking peoples, probably from the southern part of Brazil, into the southern portion of the Equatorial region.

Discussion And Overview

A map of the location of the several language groups in South America shows a very complex pattern where eight major language groups are found in about 100 large and small regions scattered over most of the continent. In addition a single small Hokan group is found on the northwest coast, but because it is so limited, it need not be discussed here. This complex pattern is very well illustrated in the map given by Joseph H.Greenberg In his "Language in the Americas" and we have chosen to examine the possible migration of peoples in South America based upon this map; we shall comment upon this source later. There may be other such maps of distributions of language groups which might serve equally as well but we are unaware of them, and the detail shown in Greenberg's map very well suits our needs. For example, he shows even very small regions of various groups, not just the major locations. One practical difficulty in the use of this map that it is illustrated in black and white, and that it is difficult to see the extent of each separate group and so identify many significant details. Thus we found it essential to produce a colored version which makes the distributions much more apparent to the human eye.

With such an aid some distinctive patterns are readily apparent. Perhaps most striking is the series of Andean speaking regions extending all the way down the west coast from Ecuador to Chile and southern Argentina. The continuity of this distribution is broken by intervening regions of some other groups as well as fairly extensive regions where the languages are either unclassified or unknown, but it is difficult to avoid the perception that the Andean languages once occupied the entire coastal region. A similar pattern is shown by Macro-Panoan which lies in three major areas extending from northern Bolivia to Uruguay with relatively small intervening regions of other groups. As with Andean, it is difficult to see that the Macro-Panoan speaking peoples did not once occupy a single region which extended from Bolivia to Uruguay.

We have unfolded the modern pattern of language groups as shown in Greenberg's map by first drawing an envelope around all members of a given group on the assumption that this represents a probable (minimum) region previously occupied by that group. That is, we have assumed that the remnants of earlier distributions are "fossilized in place". We then examined this pattern in detail, observing such features as the enclosure of an "island" of one group surrounded by a "sea" of another group and thus where the latter group has apparently penetrated a region which was earlier occupied by the former group. Another pattern which is also suggestive of such penetrations is where one group lies between significant regions occupied by another. Following this procedure we have derived a series of language distributions which show plausible earlier and later configurations which indicate a series of successive migrations, of languages and probably of people. Such "unfolding" must end when there is no indication of one language group disrupting or replacing another.

This successive unfolding leads ultimately to two or three "earliest" groups which do not overlap each other or interpenetrate to a limited extent - Andean, Macro-Panoan, and Macro-Tucanoan. Following this is a series of apparent "expansions" which are disruptive of the earliest pattern and of each other to varying degrees. The first is of a narrowly directed Chibchan-Paezan group (which we have combined here) which extends from the north to the extreme south . The next is an apparently southward movement of the Macro-Carib peoples which principally disrupted the prior Chibchan-Paezan group and the Macro-Tucanoan. The next was a vast expansion of the Equatorial group which seems to have "washed around" and through every one of the other groups and which thus extends over the largest part of the continent. The final expansion was of the Macro-Ge group which differs from the others in that it appears to have arisen and remained within the Equatorial group, disrupting it but none of the others.

These results are presented in a series of maps which we will discuss separately. For simplicity, we begin with the "earliest" patterns and successively add each of the expansions we believe have occurred.

The First Taking of the Land

We believe that it is plausible that people expanded into any adjacent, unoccupied land where their means of subsistence would allow them to live; this could mean that their existing physical equipment and way of life did not require any modifications or that changes were required. Either way, the virgin areas are likely, at least initially, to have removed or reduced restrictions on family growth due to limited food supplies and created a population increase which would encourage further expansion.

This situation may well have applied to the first three groups that we have identified as "earliest" since they show little or no interference with each other, - the Andean, Macro-Panoan, and Macro-Tucanoan. The first two present a slightly different picture from the last and we illustrate them separately.

Andean and Macro-Panoan; Map 1:

These two groups are the only ones which show no interference with each other or with other language groups and plausibly could represent the first (identifiable) occupation of South America.

In particular, the Andean group occupies most of the western coastline which is surely the region most likely to have been entered by people who (as has been proposed by many) may have followed the west coast all the way from the Bering Strait. Although coastal resources would not be unchanged in such a southward migration, there would be a generally similar environment and adaptations in equipment and way of life would not be excessive if the expansion was gradual. In Map 1 we have outlined and shaded the region which was, at one time, probably occupied by the Andean peoples; the delineation of the northern region is reasonable since there are a number of Andean regions there, but the limits of the southern region are obviously quite uncertain.

The Macro-Panoan territory is clearly not coastal since it is separated from it by the massive mountain range of the Andes. Assuming that immigration came from the north this group appears to have originated in the extreme north of Peru and the adjacent region of Brazil. Probably very significantly, this region is inland from the only region between Colombia and central Chile where the massive barrier of the Andes is broken significantly by a lower saddle of mountains. This would indicate that the forerunners of the Macro-Panoans could well have come through this saddle from the coast into the inner region of the continent. If this is so then they could originally have been coastal people, perhaps even part of the "Andean" expansion down the western coast. Presumably they then would have required some – unknown – time to adapt to the inland environment and expand toward the southeast. Thus the Macro-Panoan expansion is probably later than the Andean, though we have no means of judging what length of time this actually means.

We have outlined and shaded a wide band which the Macro-Panoan peoples may have once occupied. In a broad sense this delineation is reasonable but in particular we could plausibly have expanded the area in the south to have included much of the coastal area to the north and the south of the shaded area since there seems to be no reason why the area they occupied would be limited to that shown.

Macro-Tucanoan; Map 2:

The present Macro-Tucanoan peoples are found in widely separated areas at the headwaters of the Amazon Basin and near the Atlantic coast. We have connected and shaded the area that they might have once occupied. But, apart from this broad distribution, we also take note of three westward "intrusions" which we think are significant. The northernmost of these is determined by one small Macro-Tucanoan region between the Andean and Macro-Panoan groups. The other two project into the general area of the Macro-Panoan but cause some "necking" of the area but do not sever one from another. We see these intrusions as evidence that the Macro-Tucanoan expansion was later than the Andean and Macro-Panoan, and this seems to be a reasonable hypothesis when we consider that the adaptations needed to occupy the Amazon basin were probably considerable.

The assumption that the Macro-Tucanoan expansion proceeded from west to east is reasonable since all peoples presumably entered South America through the Isthmus of Panama. So this people had their genesis in the same general region as the Macro-Panoan, that is, northern Peru and the adjacent lands of the Amazon. We can therefore assume that this people too were descended from coastal dwellers who moved through the lower part of the Andes as did the Macro-Panoan. However the Amazon Basin surely presented an even more challenging environment than the lands to the southeast and the development of appropriate tools such as boats must surely have taken longer so that the Macro-Tucanoan would be delayed.

The presence of this third group meant that the larger part of South America was now inhabited, and all by peoples who had come from the northern part of the western coast. But we should note that there is no indication that there were people in the land bordering the northern coast or in the vast lands of southern Brazil. We shall return to this point later.

Chibchan-Paezan; Map 3:

We consider the Chibchan and Paezan people as one group and now turn to the evidence for their remarkable apparent movement from the region of the Isthmus of Panama to the southernmost part of South America.

There seems to be little doubt that this group of people came from the extreme northwest since they are not only strongly concentrated there but regions occupied by them are found in the Isthmus itself, in Mexico, and even in Florida. And so they probably represent an infusion which was not related to the previous three groups who all descended from people entering along the western coast.

One direction of their movement was southward along the west coast, quite possibly displacing or over running Andean peoples and, in the southern part isolating one Andean region from the rest. They also penetrated along the northern coast, occupying a region which, as far as we can tell, was not inhabited. But, from this northern coastal region they apparently moved due south in a fairly narrow band breaking through each of the three previously existing groups until they reached the Pacific Coast in the region of modern Chile.

Why they should do this is a mystery since they were not entering uninhabited lands and had to break through not just one but three different groups of people, all occupying different types of country. And why did they not spread out to east or west and keep pressing south? Yet that they did so seems to be the only reasonable conclusion since they left a number of still-existing Chibchan-Paezan regions in their wake. They must have had sufficient population and strength to thrust the other peoples aside, but even if there were, for example, strong population pressures, this in itself would not induce such a directed migration.

Macro-Carib; Map 4:

We now turn to the fifth movement – that of the Macro-Carib peoples – which disrupted the previous Chibchan-Paezan expansion in the north as well as the thrust to the south and further isolated the eastern regions of the Macro-Tucanoan from the western areas.

The present major areas occupied by this group are in the north and it seems reasonable that this might have been where they originated. They then would have moved west and south as we indicate in the map. Intriguingly, this is a region which we have not previously identified with any language group, but clearly, if they came from this area, it must have been inhabited. It is possible that this northern coastal area was actually occupied by Macro-Tucanoan peoples which left no residual areas where their languages were spoken. And perhaps their expansion was stimulated by the intrusion of the Chibchan-Paezan peoples into this area. These speculations are not unreasonable from our limited perspective but we can offer no clear support for them.

Equatorial; Map 5:

We now turn to the Equatorial group that is by far the most widespread of all. Its pattern is quite different from the others we have discussed and gives the impression of a vast flood which extended over most of the northern three-fourths of the country. This disrupted and displaced all of the other groups and left a large number of isolated "islands" of other groups in its wake, and it penetrated but did not entirely overwhelm the Chibchan-Paezan and Andean groups since we see small "islands" of Equatorial inside some of these regions.

The question of its origin is one that we cannot address with great confidence but examination of Map 4 shows that we have not assigned another language group to a large region in the southeast of Brazil. This seems as likely a region as any other, and, since it must have been populated if it was the source, this was presumably by peoples of the Macro-Tucanoan or the Macro-Panoan groups (see Map 2). Since the former are linked to the distinctive Amazonian ecology we incline to the opinion that the Macro-Panoan peoples were the forerunners of the Equatorial peoples.

Macro-Ge; Map 6:

The distribution of the last group, Macro-Ge, is different from the others since it is essentially confined within the Equatorial group. It contains many islands of Equatorial speakers, but simply abuts a few regions of Macro-Carib and Macro-Tucanoan without surrounding them. Perhaps it originated from within the Equatorial peoples or possibly from an enclave of their speculative forerunners, the Macro-Panoan.

Concluding Comments

We believe that the essential correctness of our analysis is well demonstrated by the fact that the complex pattern of language distributions in South America can be reproduced by a small, plausible, series of movements of peoples. If the basic assumptions and our unfolding were severely in error it is surely would show itself, but nothing of this kind is apparent. While the simplicity of our analysis is probably its most convincing feature this clearly does not mean that these few large-scale movements are all that is needed to understand the pattern of languages in South America. They appear to provide the basic underlying pattern, but significant "local" movements of people must surely have also occurred.

This analysis is only feasible because of the fact that the boundaries of earlier, large, expansions of peoples is apparently largely preserved in sometimes isolated, often small, regions. In our phrase, fragments of the previous structures are essentially "fossilized in place". This is truly remarkable, but no more so, for example, than postulating that small groups of people migrated very long distances through regions occupied by other language groups. And we should note that we can not assume that some remoter regions did not survive to be "fossilized".

And our analysis presents some unanswered questions. One such is the reality of the strikingly narrow Chibchan-Paezan migration to the south through the center of the continent. This seems to be difficult to understand as due to population pressure or improved mastery of food gathering techniques, and, since it is a question of what motivated peoples in the past it is probably unanswerable by us now. Other questions are those of the origins of the Macro-Carib and

Equatorial peoples; did they arise where we have postulated, and from earlier groups?

Some of these questions can be addressed by linguistic analysis since our work implies a set of linguistic relationships. This we illustrate roughly below; the earlier groups are listed first and possible relationships are implied by presence in a column. The Chibchan-Paezan stands alone, and Andean may be a forerunner of both Macro-Panoan and Macro-Tucanoan.

Andean

Macro-Panoan

Macro-Tucanoan

Chibchan-Paezan

Macro-Carib ?

Equatorial ?

Macro-Ge

Since we imply a time-sequence in this chart we would like to note that we have no guidance on the actual times involved. The individual expansions may perhaps have taken only hundreds of years, but the interval between one expansion and the next could be several thousands rather than hundreds of years. And is it conceivable that the Andean peoples are actually remnants of the first entrants into South America, tens of thousands of years ago?

Last, but by no means least, we believe that it is appropriate to comment on the validity of the source data, that is, Greenberg's evaluation of language groups. The author is in no position to evaluate the linguistic arguments that led to Greenberg's assessment, but it is clear that there are many linguists that disagree strongly with his views. Whatever the validity of these criticisms the author finds it very difficult to believe that a map which contains gross inaccuracies could be unfolded as we have done without showing numerous anomalies and contradictions. These are not apparent in our analysis and so our work is, in some sense, support for the overall validity of Greenberg's analysis of South American languages. This comment clearly only applies to the South American languages, and not to other views that are presented on American languages as a whole.

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